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THE PEDAGOGICS OF PATHOLOGY[1]

IN conformity with the established precedent that the presiding office shall be holden for some remarks in inaugurating his term of service, I venture to present some reflections on the pedagogics of pathology. As members of this society, and of the medical profession, we are all interested in the question of medical education, whether engaged or not in actual teaching. In the curriculum of the medical school, pathology occupies a position of especial interest and importance in relation to the other branches. More than any other topic it bridges the gap which at the present time exists between the sciences fundamental to medicine and the clinical subjects.

Pedagogics—the science of teaching methods—has been making rapid progress in the last half century, and has come to occupy a place among the recognized sciences.

Modern pedagogy is of necessity a recent growth, because experimental psychology itself, upon which it is based, is barely forty years old. The first laboratory for experimental psychology was established by Wundt in Leipsic about 1876, while in this country no such laboratory existed until the one opened by Dr. G. Stanley Hall, at the Johns Hopkins University about 1888. Previous to this time, however, important contributions had been made to the teaching art and the ideas of Pestalozzi, of Herbart, of Rousseau, and others were gradually influencing our educational methods. The importance of laboratory or objective methods, the individual-

[1] Address of the President of the Chicago Pathological Society, October 11, 1915.

ization of teaching by closer attention to the peculiarities of the individual pupil, the splitting of large classes into smaller and smaller groups, the elective plan, the continuous session, best exemplified in the quarter system, research as an essential function of the university and as a pedagogic method, the division of the sciences, particularly, into smaller subdivisions, specialization in a word—these and other pedagogic innovations were, and still are, receiving attention from educators, and are being adopted throughout our educational system from the kindergarten onward. Of special importance were the introduction of objective methods of teaching, in field and laboratory, and the recognition of the individual differences of pupils, with the consequent necessity of adjusting the educational process to these differences by some scheme of election or selection of topic, method and teacher in order to secure the best results.

Laboratory methods began to be introduced into the American colleges—on any extensive scale—not much more than half a century ago, at first in chemistry, then in physics and later in the several biologic branches. The supplanting or supplementing of the older didactic methods of lecture and recitation by these laboratory exercises extended rather rapidly in the literary college, but they made their way much more slowly in the medical school. It might have been expected that the contrary would have been the case when it is recalled that for a century or more one laboratory or practical course, that of human dissection, had occupied a recognized place in the medical curriculum, and was required of every student, but it is barely thirty years since any other laboratory course was generally required in the medical college. It is but little over thirty years since the writer attended the two five-months courses of lectures at that time required for graduation in a medical college of the middle west. Coming from a small university, where even at that time the laboratory exercise constituted the most important feature of instruction in all of the sciences, it seemed strange to find no practical work whatever, aside from dissection and a brief four or five weeks' superficial course in chemistry. A year later, having discovered that he was inadequately prepared, it was decided that he should take another course of lectures, and the choice of an eastern medical college was determined by the fact that it alone, of all of the medical colleges in this country, offered a practical or laboratory course in connection with each of the seven departments—chairs, as they were called—which made up the medical college of that day, namely, anatomy, physiology, chemistry, materia medica, medicine, surgery and obstetrics. Most of these practical courses were brief and superficial, consisting in large part of demonstrations by the teacher rather than of individual work by the student, but they were the beginning of better things.

The explanation of the tardy adoption by the medical faculty of these modern methods is to be found in the fact that its members were not primarily educators, and they were therefore not keeping closely in touch with the advances in pedagogic methods. They were for the most part, even in the fundamental branches, practitioners of medicine for whom the giving of medical lectures two to four times a week during the five or six winter months was an incidental avocation. Not until the medical schools came to have a real, vital connection with the university, and its faculty was brought in close contact with, and came, in part, to be composed of, men who were making teaching the chief, if not the sole business of their lives, was any considerable

progress made in the introduction of more rational and effective methods.

Pathology in my student days was not recognized as an essential subject in the medical curriculum. Indeed at the first college I attended it was not only not taught, but was ridiculed by some of the most influential members of the faculty, as "Dead-house Medicine." In the second college mentioned, perhaps at that time the most progressive medical college in this country, a few incidental exercises, by a subordinate teacher, who presented some post-mortem specimens to the class, comprised all that was offered. How great has been the advance in the subsequent quarter century is in no wise better illustrated than by the number and character of the contributions made by undergraduate students to the transactions of this society.

Meanwhile, pathology itself has been extending its scope very rapidly. The term pathology no longer stands for mere pathological anatomy, the study of the end results of disease but has come to signify, as its etymology implies, a comprehensive study of the phenomena of disease in the living, the behavior of cells, tissues, organs and systems, in an abnormal environment, or their reaction to abnormal stimuli—an exhaustive search for the causes of these abnormalities, physical, chemical and biologic, and the rationale of the processes by which these etiologic factors bring about the phenomena which we call disease. Indeed, some of the more recent text-books on medicine include, under the heading of pathology, the discussion of etiology, symptomatology, pathogenesis and pathologic anatomy, everything, in short, but diagnosis, prognosis and therapy.

The methods by which this comprehensive knowledge is sought are more and more becoming the methods of exact, carefully controlled experiment on animals. The mere study of post-mortem changes, important though it is, comes yearly to occupy a relatively less important rôle in the task of the pathologist. Infection and immunity, serology, protozoology, chemical pathology, these and other phases of the subject, newly born in the last decade or two, are rapidly changing our whole conception of pathology, are greatly enlarging its scope, and are, as it seems to me, necessitating a constant readjustment of this branch to other branches in the medical curriculum, and improvements in our methods of instruction by which the coming generation of practitioners must be educated in pathology.

The pedagogics of pathology involves a consideration of (1) its relative importance in the curriculum—that is, the number of scheduled hours that should be assigned to the subject; (2) its proper sequence in the program of courses to be determined by the prerequisites which are essential for its intelligent study, and by its relation to the more advanced clinical subjects for the pursuit of which some knowledge of pathology is necessary, and (3) the methods to be employed in the teaching of the subject, and this depends largely on one's conception of the scope and content of pathology as a science.

A survey of the curricula of about forty of the leading university medical schools of the United States discovers the fact that while there are considerable variations in the courses in pathology offered in these schools, as to the three points just mentioned, on the whole the differences are less marked than are those in the clinical subjects.

The amount of time assigned to the subject is somewhat difficult to determine, as some of the topics taught in most schools under the general heading of pathology, are offered in others in connection with bacteriology, while other topics, for example,

laboratory or clinical diagnosis, are taught in the department of medicine or in other clinical departments. As nearly as can be estimated the time allotted to pathologic topics ranges from 210 hours in one school to a maximum of 504 hours in the University of Ohio.

One of the most conclusive evidences that up to 8 or 10 years ago the principles of pedagogy were little understood or regarded by medical faculties, was the great diversity in the number of hours assigned to the several medical branches. You will recall the rather startling findings of the investigations made as to this matter a decade ago by the secretary of the Michigan State Board of Medical Examiners, the president of the Illinois State Board of Health, and by the Council on Medical Education of the American Medical Association. It was found that the number of scheduled hours assigned to some of the important medical subjects varied by as much as three or four hundred per cent., and that some of the minor subjects, for example, orthopedic surgery, occupied as much of the student's time in some colleges as a major branch like medicine or general surgery.

It is obvious that these curricula were not prepared with any logical consideration of the relative values of the medical branches. Each instructor, impressed with the magnitude of his own subject, clamored for more and more time, insisting that he could not "cover the ground" of his branch of medicine in any number of hours that could in reason be given over to it in a four-year course of study. The construction of a medical curriculum from the point of view of the average student's working capacity and the logical division of his total working hours between the several branches in ratio to their relative importance to the medical practitioner seems not to have been attempted until a very recent period.

When the problem is approached from this standpoint it becomes at once evident that by no possible scheme can such a number of scheduled hours be allotted to any branch as will make it possible for the instructors to completely "cover the ground" of that branch. The task of each department is to make such use of the number of hours assigned to it, by faculty vote, as will secure to each student the largest possible educational result for the expenditure of that time in the given topic. With a curriculum blocked out on such a basis, pathology can not logically demand more than 8 or 9 per cent.—together with bacteriology not to exceed 12 per cent.—of the total number of scheduled hours in the four years' course of study; as this total in the average medical school is approximately 4,400 hours, the time assigned to pathology should not exceed 350 or 400 hours.

I find that the average allotment to the courses in pathology in the curricula of the forty colleges which I have examined is about 350 hours, although there are a few marked exceptions. The time allotted to bacteriology ranges from about 100 to 200 hours, with an average of 150, thus making a total for the two subjects fairly proportionate to their importance in relation to the other branches of the medical curriculum.

The logical sequence of the medical subjects is determined by certain fundamental pedagogic principles.

The study of structure logically precedes the study of function; knowledge of the normal is essential to an understanding of the abnormal, although the study of the latter often yields valuable information about the normal; a broad knowledge of the principles, facts and methods of any growing science to specific problems, as those of medicine, is an indispensable prerequisite if the student is to be so grounded

in that science as to be able to keep abreast with its progress after his student days. It is a fatal error to present the fundamental medical sciences with too exclusive reference to their practical application to medicine and surgery, although the occasional illustration of the bearing of anatomy, physiology, pathology, etc., on their practical application to clinical medicine is of distinct advantage. With these principles in mind the position of pathology in the medical curriculum is not difficult to determine. There is a rather striking uniformity in the place assigned to bacteriology and pathology in the courses of study in the several colleges. In the majority of colleges bacteriology is offered in the first semester of the second year, followed by general pathology in the second semester, and by special pathology, with autopsies, in the third year. In a few institutions bacteriology is given in the first year, and in two instances pathology proper is begun at this time. In a number of colleges advanced pathology with autopsies, and special topics such as surgical, gynecologic, neurologic and dermatologic pathology are continued into the senior year.

Bacteriology, it seems to me, might, with advantage, be divided into two parts, general bacteriology, with laboratory technique, being given in the first year, or better still, required for admission, to be followed by a briefer course in the pathogenic bacteria, and in infection and immunity, in the second year—preferably the first semester. The fundamental course is an excellent one for the beginning student, no work in the other sciences being prerequisite for the intelligent study of the subject, and the laboratory technique affording a good training for the laboratory exercises in the other branches. It seems impracticable to crowd such a course into the two years of premedical work at present demanded by most schools, but it is probable that this minimum amount can, before long, be extended to three years, and thus provide time for bacteriology.

The position of general pathology in most curricula is as it should be. It secures to the student the necessary preparation in anatomy, gross and microscopic, embryology, physiology, biologic chemistry and bacteriology, and, on the other hand, links these subjects closely to the clinical branches. I would urge, however, the importance of placing pathologic anatomy with autopsies later than is now done in many schools. The student should have acquired some considerable first-hand knowledge of the phenomena of disease in the living patient before he studies the final outcome of serious disease as disclosed at the autopsy, and as often as possible he should follow the individual patient, dying from some affection, which he has observed in life, to the post-mortem table. Of especial educational value is the study of a few cases, seen at autopsy, exhaustively, by every feasible method of examination, as contrasted with a second-hand survey of the whole field of pathologic anatomy by lecture, text-book and recitation. One of our members, not long since, described his own method of conducting such a course, which seems to me in every way commendable.

May I, in this connection, comment on the singular and widely prevalent perversity with which teachers in all lines and grades of education in this country are prone to get away, in their practise, from the fundamental idea and purpose of education?

Its primary purpose is to develop the mental facilities, *not* to impart information. And this is quite as true of medical as of any other kind of education. That the pupil shall be educated, trained to observe

accurately, and thoroughly, to record his observations clearly and concisely, and to reason logically from his findings to sound deductions—*that* is the main purpose and function of any educational process, be it medical, pathologic or any other. We in America seem especially obtuse in our failure to recognize this vital truth, as is shown conspicuously in the character of our examinations. We have made some progress in our teaching methods, though we have yet much to learn, but we continue to test the results of our work by methods a half century out of date. In examinations for graduation, for licensure to practise (with exceptions now in a few states) and in competitive examinations for interneships in the best hospitals, the papers set are wholly written papers of such sort as to test only the volume of facts which the candidate has been able to cram into his cranium—a test, in other words, of but one mental faculty, the memory, and that by no means the most important. Into the student's real power to see and hear and feel things, to know what he observes, and to draw sound conclusions from his findings—in short his ability to *do* the things he is to be called upon to do as an interne and a practitioner of medicine, we make no real inquiry whatever. If we are to give our students the proper conception of what they are attending the medical school for, and an effective stimulus to really develop all of their mental faculties, along medical lines, we must radically change the character of our examinations.

The teachers of pathology are in an especially advantageous position to set an example in this regard, and to exert a profound influence for the betterment of our methods of examination.

Another serious mistake is the growing tendency to crowd some of the clinical subjects into the sophomore year. In one school, for example, courses in medicine, surgery, obstetrics, ophthalmology and dermatology now occupy a considerable portion of the time of the second semester of the sophomore year; at another, the sophomores take medicine, both clinical and didactic, surgery and pediatrics; at a third school, medicine, surgery, obstetrics and dermatology occupy 10 hours weekly of the 25 or 30 available, while in yet another, medicine, surgery and obstetrics are begun at this time. The time available for the fundamental sciences of anatomy, general pathology, etc., is all too short when the full time of the two years is devoted exclusively to them. The education which the student is obtaining in these fundamental subjects in these two years is the only systematic instruction he will ever get in those branches, and must serve him for a life time. Most students leave the medical college at graduation with a resolve to review these branches from time to time, but very few indeed ever do so. Deficiencies in clinical training are readily amended, because the subjects are right in line with the physician's daily work; shortcomings in the fundamental branches can be corrected only at a considerable sacrifice of time, money and energy.

I have indicated that I would lay much less stress on pathologic anatomy than has been and still continues to be the custom in most schools, and more, increasingly more, time on experimental pathology, chemical pathology, serology, and the other newer developments of the subject. As to pathologic anatomy much can be done to familiarize students with the gross and microscopic appearance of diseased tissues and organs (and this with the minimum expenditure of effort by both student and teacher) by having displayed in a well-lighted central place, frequented by the student body for study or lounging, patho-

logic specimens, plates, drawings, charts and other illustrative materials. In idle moments one can accumulate a large amount of information by this means. It is the height of stupidity to lock up material of this sort in dark inaccessible cases, to be brought forth only on special occasions.

There is need, in most schools, of a better correlation between the department of pathology and the clinical departments. The clinical teachers are, for the most part, busy practitioners having little time, even when they have the inclination, to keep in touch with what is going on in the pathological department so as to command the resources of its museum in their clinical teaching. The instructors in pathology can accomplish a great deal to secure more thorough and accurate presentation of the pathologic side of the clinical subjects if they are constantly calling the attention of the clinicians to the materials in their museums and laboratories, and are themselves available at convenient hours to respond to a summons to the medical, surgical or other clinic to demonstrate pathologic specimens, illustrative of the topic being presented, or the clinical patient who is being shown, and to explain the newer, more refined laboratory methods of diagnosis.

No colleague of mine would think it possible for me to get through a discussion on an educational topic without allusion to the elective system. I should be sorry to disappoint any one, or to lose such an opportunity. May I say then first, that after fifteen years of experience and observation I am more confirmed in my belief than ever before, of its applicability to, and its advantage, with suitable restrictions, in medical education. The elective principle has been adopted, and is in operation, in one form or another, in every college and university of any importance in this country. It has been an integral part of the educational system in the German, and most other foreign universities, in the medical as well as in other departments, for over fifty years. I have elsewhere described the plan and discussed its advantages in the medical curriculum at length, and do not here purpose to review that discussion. However, to correct certain erroneous impressions, which seem to be quite prevalent, may I say that the elective system, properly administered, does not mean the unrestricted freedom of the student to do just as he pleases—to choose any course of study which strikes his fancy. Its chief advantages do *not* lie in the choice of branches or even of topics, but much more in the choice of methods of study—by lecture, recitation, laboratory, clinic, research—and of the instructor under whom each course is to be pursued. Its purpose in the medical school is *not*, or should not be, to encourage or even to permit, the undergraduate student to follow a restricted course of study in preparation for a special line of practise, although in this form it is in operation, mistakenly, as I believe, in two or three of our leading medical schools. It does *not* mean, for example, that any student should be permitted to neglect pathology, or medicine or any special clinical subject. It *does* make for his opportunity to get the minimum amount to be required of that subject by the method and under the instructor that will insure for him the best training, that is the maximum educational gain in that branch of study, and it should permit and encourage him to pursue that subject much beyond the minimum amount required for graduation, if he finds therein, as very many students do, the best medium for developing his powers of observation and reasoning.

As surely as individuals differ, as they do differ widely, in mental equipment and

capacity, just so surely can we obtain the best results in education only by such elasticity in our methods as makes possible the adjustment of the educational procedure to each student's capacity, peculiarities and needs.

And finally, I would speak for a much wider adoption and extension of the method of research in medical education. The daily practise of medicine, for which we are preparing most of our pupils, is research of the highest order and the most difficult type. We train the neuro-muscular apparatus and the special senses to efficiency in any particular direction, by their constant exercise in that activity or direction. How can we better train the mental facilities for research at the bedside than by their exercise in research, in laboratory and clinic? No one would deny that a certain body of fundamental facts and principles must be memorized by every medical student, and facility in certain technical procedures ought to be acquired, but if we hope to arouse in the student a real enthusiasm for his work, and to develop his power of independent initiative and accomplishment in the setting and the solving of problems, it can be done only—certainly most effectively—by setting him to the task of solving problems within his capacity, involving factors within his control, his work being carried on under intelligent, wise supervision. The problems of pathology are peculiarly well adapted for this purpose. They may be so selected as to have to do with materials and factors within the scope of his ability and training, and they are of interest to him because he can readily see their bearing on clinical medicine, for which he feels he is fitting himself. If he is to have zeal in their pursuit, however, it must not be the threshing over of old straw, but new problems whose solution he feels may constitute a real contribution, however small, to medical science. If he can then present the results of his efforts to a dignified, earnest group of his elders, such as assemble at the meetings of this society, it means for him a generation of enthusiasm, a development of real power, such as no other educational method can produce.

This society has great reason to be proud of the subsequent work of many men and women who found here their first opportunity and their best inspiration. It is certain to have increasing cause for gratification, as the years go on, in the results of this phase of its activities.

John Milton Dodson

PLANT MORPHOLOGY[1]

I propose to deal with some aspects of the study of plant morphology. In doing so I shall not accept any definition of morphology that would separate it artificially from other departments of botany. I regard the aim of plant morphology as the study and scientific explanation of the form, structure, and development of plants. This abandons any sharp separation of morphology and physiology, and claims for morphology a wider scope than has been customary for the past fifty years. During this period the problem of morphology has been recognized as being "a purely historical one," "perfectly distinct from any of the questions with which physiology has to do," its aim being "to reconstruct the evolutionary tree." The limitation of the purpose of morphological study, expressed in these phrases from the admirable addresses delivered to this section by Dr. Scott and Professor Bower some twenty years ago, was due to the in-

[1] From the address of the president of the Section of Botany, Manchester meeting of the British Association for the Advancement of Science.

fluence of the theory of descent. I fully recognize the interest of the phyletic ideal, but am unable to regard it as the exclusive, or perhaps as the most important, object of morphological investigation. To accept the limitation of morphology to genealogical problems is inconsistent with the progress of this branch of study before the acceptance of the theory of descent, and leaves out many of the most important problems that were raised and studied by the earlier morphologists.

In the history of morphology, after it had ceased to be the handmaid of the systematic botany of the higher plants, we may broadly distinguish an idealistic period, a developmental period, and a phyletic period. The period of developmental morphology, the most fruitful and the most purely inductive in our science, was characterized by an intimate connection between morphological and physiological work. Among its contributions were studies of development or "growth histories" of whole plants and their members. These were carried out, in part at least, in order to investigate the nature of development, and such general problems found their expression at the close of the period in the "Allgemeine Morphologie" of Hofmeister. The "Origin of Species" took some years before it affected the methods and aims of botanical work. Then its effect on morphology was revolutionary, and, as in all revolutions, some of the best elements of the previous *régime* were temporarily obscured. This excessive influence of the theory of descent upon morphology did not come from Darwin himself, but from his apostle Haeckel, who gave a very precise expression to the idea of a genealogical grouping of animals and plants, illustrated by elaborate hypothetical phylogenetic trees. Such ideas rapidly dominated morphological work, and we find a special "phylogenetic method" advocated by Strasburger. The persistence of the phyletic period to the present time is shown, not only in the devotion of morphology to questions of relationship, but in the attempts made to base homologies upon descent only. Lankester's idea of homogeny can be traced to the influence of Haeckel, and nothing shows the consistency of phyletic morphology to its clear but somewhat narrow ideal so plainly as the repeated attempts to introduce into practise a sharp distinction between homogeny and homoplasy.

Professor Bower, in his address last year and in other papers, has dealt illuminatingly with the aims and methods of phyletic morphology. I need only direct attention to some aspects of the present position of this, which bear on causal morphology. The goal of phyletic morphology has throughout been to construct the genealogical tree of the vegetable kingdom. In some ways this seems farther off than ever. Phyletic work has been its own critic, and the phylogeny of the genealogical tree, since that first very complete monophyletic one by Haeckel, affords a clear example of a reduction series. The most recent and trustworthy graphic representations of the inter-relationships of plants look more like a bundle of sticks than a tree. Consider for a moment our complete ignorance of the inter-relationships of the Algæ, Bryophyta, and Pteridophyta. Regarding the Algæ we have no direct evidence, but the comparative study of existing forms has suggested parallel developments along four or more main lines from different starting-points in a very simple unicellular ancestry. We have no clue, direct or indirect, to the ancestral forms of the Bryophyta, and it is an open question whether there may not be as many

parallel series in this group as in the Algæ. The Pteridophyta seem a better case, for we have direct evidence from fossil plants as well as the comparison of living forms to assist us. Though palæobotany has added the Sphenophyllales to the existing groups of vascular cryptogams and has greatly enlarged our conceptions of the others, there is no proof of how the great groups are related to one another. As in the Bryophyta, they may represent several completely independent parallel lines. There is no evidence as to what sort of plants the Pteridophyta were derived from, and in particular none that relates them to any group of Bryophyta or Algæ. I do not want to labor the argument, but much the same can be said of the seed-plants, though there is considerable evidence and fairly general agreement as to some Gymnosperms having come from ancient Filicales. The progress of phyletic work has thus brought into relief the limitations of the possible results and the inherent difficulties. As pointed out by Professor Bower, we can hope for detailed and definite results only in particularly favorable cases, like that of the Filicales.

The change of attitude shown in recent phyletic work towards "parallel developments in phyla which are believed to have been of distinct origin" is even more significant. Prof. Bower spoke of the prevalence of this as an "obstacle to success," and so it is if our aim is purely phyletic. In another way the demonstration of parallel developments constitutes a positive result of great value. Thus Professor Bower's own work has led to the recognition of a number of series leading from the lower to the higher Filicales. By independent but parallel evolutionary paths, from diverse starting-points in the more ancient ferns, such similarity has been reached that systematists have placed the plants of distinct origin in the same genus. In these progressions a number of characters run more or less clearly parallel, so that the final result appears to be due "to a phyletic drift that may have affected similarly a plurality of lines of descent." This conclusion, based on detailed investigation, appears to me to be of far-reaching importance. If a "phyletic drift" in the ferns has resulted in the independent and parallel origin of such characters as dictyostely, the mixed sorus, and the very definite type of sporangium with a vertical annulus and transverse dehiscence, the case for parallel developments in other groups is greatly strengthened. The interest shifts to the causes underlying such progressive changes as appear in parallel developments, and the problem becomes one of causal morphology rather than purely historical.

The study of parallel developments would, indeed, seem likely to throw more light on the morphology of plants than the changes traced in a pure phyletic line, for it leads us to seek for common causes, whether internal or external. We cease to be limited in our comparisons by actual relationship, or forbidden to elucidate the organization in one group by that which has arisen independently in another. Similarly the prohibition against comparing the one generation in the life-cycle with the other falls to the ground, quite apart from any question of whether the alternation is homologous or antithetic. The methods of advance and the causal factors concerned become the important things, and if, for example, light is thrown on the organization of the fern-plant by comparison with the gametophyte of the moss, so much the better. This, however, is frankly to abandon phylogeny as "the only real basis of morphological study,"

and with this any attempt to base homology on homogeny. Many of the homologies that exist between series of parallel development are what have been happily termed homologies of organization; these are sometimes so close as to result in practical identity, at other times so distinct as to be evident homoplasies. The critical study of homologies of organization over as wide an area as possible becomes of primary interest and importance.

Since about the beginning of the present century a change of attitude towards morphological problems has become more and more evident in several ways. It seems to be a phyletic drift affecting simultaneously a plurality of lines of thought. The increasing tendency to look upon problems of development and construction from a causal point of view is seen in the prominence given to what may be termed developmental physiology, and also in what Goebel has called organography. These deal with the same problems from different sides and neither formulates them as they appear to the morphologist. Together with genetics, they indicate the need of recognizing what I prefer to call general or causal morphology.

The problems of causal morphology are not new, though most of them are still unsolved and are difficult to formulate, let alone to answer. As we have seen, they were recognized in the time of developmental morphology, though they have since been almost wholly neglected by morphologists. So far as they have been studied during the phyletic period, it has been from the physiological rather than the morphological side. Still, such problems force themselves upon the ordinary morphologist, and it is from his position that I venture to approach them. I willingly recognize, however, that causal morphology may also be regarded as a department of plant-physiology. In development, which is the essential of the problem, the distinction between morphology and physiology really disappears, even if this distinction can be usefully maintained in the study of the fully developed organism. We are brought up against a fact which is readily overlooked in these days of specialization, that botany is the scientific study of plants.

General morphology agrees with physiology in its aim, being a causal explanation of the plant and not historical. Its problems would remain if the phyletic history were before us in full. In the present state of our ignorance, however, we need not be limited to a physico-chemical explanation of the plant. Modern physiology rightly aims at this so far as possible, but, while successful in some departments, has to adopt other methods of explanation and analysis in dealing with irritability. It is even more obvious that no physico-chemical explanation extends far enough to reach the problems of development and morphological construction. The morphologist must therefore take the complicated form and its genesis in development and strive for a morphological analysis of the developing plant. This is to attack the problem from the other side, and to work back from the phenomena of organization toward concepts of the nature of the underlying substance.

It is to these questions of general morphology with a causal aim (for causal morphology, though convenient, is really too ambitious a name for anything we yet possess) that I wish to ask your attention. All we can do at first is to take up a new attitude towards our problems, and to gather here and there hints upon which new lines of attack may be based. This new attitude is, however, as I have pointed

out, a very old one, and in adopting it we re-connect with the period of developmental morphology. Since the limited time at my disposal forbids adequate reference to historical details, and to the work and thought of many botanists in this field, let me in a word disclaim any originality in trying to express in relation to some morphological problems what seems to me the significant trend, in part deliberate and in part unconscious, of morphology at present. The methods available in causal morphology are the detailed study in selected plants of the normal development and its results, comparison over as wide an area as possible, with special attention to the essential correspondences (homologies of organization) arrived at independently, the study of variations, mutations, and abnormalities in the light of their development, and ultimately critical experimental work. This will be evident in the following attempt to look at some old questions from the causal point of view. I shall take them as suggested by the fern, without confining my remarks to this. The fern presents all the main problems in the morphology of the vegetative organs of the higher plants, and what little I have to say regarding the further step to the seed-habit will come as a natural appendix to its consideration.

Twice in its normal life-history the fern exhibits a process of development starting from the single cell and resulting in the one case in the prothallus and in the other in the fern-plant. For the present we may treat these two stages in the life-history as individuals, their development presenting the same general problems as a plant of, say, Fucus or Enteromorpha, where there is no alternation of generations. How is the morphologist to regard this process of individual development?

In the first place, we seem forced to regard the specific distinctness as holding for the germ as well as the resulting mature plant, however the relation between the germ-cell and the characters of the developed organism is to be explained. We start thus with a conception of specific substance, leaving it quite an open question on what the specific nature depends. This enables us to state the problem of development freed from all considerations of the ultimate uses of the developed structure. The course of development to the adult condition can be looked upon as the manifestation of the properties of the specific substance under certain conditions. This decides our attitude as morphologists to the functions of the plant and to teleology. Function does not concern us except in so far as it is found to enter as a causal factor into the process of development. Similarly, until purpose can be shown to be effective as a causal factor it is merely an unfortunate expression for the result attained.

Let me remind you, also, that the individual plant, whether it be unicellular, cœnocytic or multicellular, may behave as a whole at all stages of its development. We see this, for instance, in the germination of *Œdogonium*, in the germination and subsequent strengthening of the basal region in *Fucus* or *Laminaria*, in the moss-plant or fern-plant, or in a dicotyledonous tree. A system of relations is evident in the plant, expressed in the polarity and the mutual influences of the main axis and lateral branches, as well as in the influences exerted on the basal region by the distant growing parts. We thus recognize, in its most general form, the correlation of parts, a concept of proved value in botany.

To some the expression of the observed facts in this way may appear perilously mystical. I do not think so myself. It is true that the nature of the specific substance and of the system of relations is un-

known to us, but it is regarded as a subject for scientific inquiry and further explanation. To recognize fully the complexity of the substance of the plant is not, however, a step towards neo-vitalism, but is perhaps our best safeguard against the dangers of this.

The wholeness of the individual, together with important phenomena of regeneration, has suggested the conclusion that something other than physico-chemical or mechanical laws are concerned in the development of the organism. To this something Driesch applies the name entelechy. Without discussing the vitalistic philosophy of the organism, or other modern phases of philosophic thought that treat life as an entity, it seems worth while to point out that they are based mainly on the consideration of animal development. It would be interesting to inquire into the difficulties that are met with in applying such views to plants, where regeneration in one form or another is the rule rather than the exception, and often does not lead to restitution of the individual. Causal morphology can recognize phenomena of development and of the individual, which are at present beyond physico-chemical explanation, and try to attack them by any methods of investigation that seem practicable, without begging the main question at the outset and then proceeding deductively. To assume any special inner director of development. be it entelechy or vital force, is to cut the knot that may ultimately be untied.

The previous experience of botany in the time of nature-philosophy may well make us cautious of solving our difficulties by the help of any new biological philosophy. On the other hand, cooperation between biology and philosophic thought is highly desirable. In this connection I should like to refer to an idea contained in Prof. Alexander's paper on the basis of realism. He suggests that there is only one matrix from which all qualities arise, and that (without introducing any fresh stuff of existence) the secondary qualities, life, and at a still higher level, mind, emerge by some grouping of the elements within the matrix. The development of this idea as it applies to life would appear to offer a real point of contact between inductive biological work and philosophy.

To return to our plant, its development, with increase in size and progressive complexity of external form and internal structure, must be considered. The power of continued development possessed by most plants and wanting in most animals makes comparison between the two kingdoms difficult. That there is no fundamental difference between the continued and the definitely limited types of embryogeny is, however, shown by plants themselves. The bryophyte sporogonium is a clear example of the latter, while the fern sporophyte is one of many examples of the former. A difference less commonly emphasized is that in the sporogonium (as in the higher animals) the later stages of development proceed by transformation of the whole of the embryo into the mature or adult condition; in the fern-plant the apical development results in successive additions of regions which then attain their mature structure by transformation of the meristematic tissue.

These distinctions are of some importance in considering the generalization originally founded on animal development and known as the biogenetic law. That "the ontogeny is a concise and compressed recapitulation of the phylogeny" is essentially a phyletic conception. It has been more or less criticized and challenged by some distinguished zoologists, and has always been difficult to apply to plants. If we avoid being prejudiced by zoological theory and results, we do not find that the characters of the em-

bryos of plants have given the key to doubtful questions of phylogeny. What help do they give us, for instance, in the algæ or the vascular cryptogams? The extension of the idea of recapitulation to the successively formed regions of the seedling plant requires critical examination; if admitted it is at any rate something different from what the zoologist usually means by this. The facts—as shown, for instance, in a young fern-plant—are most interesting, but can perhaps be better looked at in another way. Development is accompanied by an increase in size of the successively formed leaves and portions of stem, and the process is often cumulative, going on more and more rapidly as the means increase until the adult proportions are attained. The same specific system of relations may thus find different expression in the developing plant as constructive materials accumulate. I do not want to imply that the question is merely a quantitative one; quality of material may be involved, or the explanation may lie still deeper. The point is that the progression is not a necessary one due to some recapitulative memory.

There are some other classes of facts, clearly cognate to normal individual development, that seem to require the causal explanation. I may mention three: (1) Vegetatively produced plants (from bulbils, gemmæ, etc.) tend in their development to pass through stages in elaboration similar to young plants developing from a spore or zygote. The similarities are more striking the smaller the portion of material from which a start is made. (2) Branches may repeat the stages in ontogeny more or less completely also in relation to differences in the nutritive conditions. (3) In the course of continued development there may be a return to the simpler form and structure passed through on the way to the more complex. These cases of parallels to, or reversals of, the normal ontogenetic sequence suggest explanation on causal lines, but are difficulties in the way of phyletic recapitulation; the first two cases can be included under this, while the third seems definitely antagonistic. On the whole, it may be said that recapitulation can not be accepted for plants without further evidence, and that preliminary inquiry disposes us to seek a deeper and more fruitful method of explaining the facts of development.

The development of most plant-individuals starts from a single cell, and when we compare mature forms of various grades of complexity the unicellular condition is also our usual starting-point. What is not so generally recognized or emphasized is the importance of the filament as the primitive construction-form of most plants. I do not use the word primitive in a phyletic sense, nor in the sense of an ideal form, but to indicate a real stage in independent progressions underlying many homologies of organization. I can not develop this fully here, but wide comparison of independent lines of advance suggests that the main types of progress in complexity of the plant-body have involved the elaboration of the single filament with apical growth and with subordinated "branches." It is generally recognized that various groups of algæ show how a solid multicellular axis may come about, not only by the further partition of the segments of the apical cell, but by the congenital cortication of a central filament or the congenital condensation of the subordinated "branches" on to the central axis. The algæ further show the change from the dome-shaped apical cell of a filament to the sunken initial cell with two, three or four sides. The central filament then only appears, if at all, as a subsequent differentiation in the tissue, and the segments serially cut off from the apical cell may or may not bear projecting hair-shoots

or "leaves." The algæ thus attain in independent lines a construction corresponding to that of the plant in liverworts and mosses. In the various parallel series of Bryophyta the filament is not only more or less evident in the ontogeny, but may be regarded as the form underlying both thallus and shoot, between which on this view there is no fundamental distinction. The sporogonium also can be readily regarded as an elaborated filament. While the same interpretation of the fern-prothallus will readily be granted, to think of the fern-plant as the equivalent of an elaborated filament may appear far-fetched. So far from this being the case, I believe that it will be found helpful in understanding the essential morphology of the shoot. In a number of vascular cryptogams and seed-plants, there is actually a filamentous juvenile stage, the suspensor, while the growth by a single apical cell is essentially the same in the fern as in the moss and some algæ.

There follows from this a natural explanation of the growth by a single initial cell so commonly found in plants. The apical cell appears to be the one part of the massive plant-body (for instance, of Laurencia, a moss, or a fern) that persists as a filament; it is a filament one cell long. It may be replaced by a group of initial cells, as we see in some algæ, liverworts and Pteridophyta, and this leads naturally to the small-celled meristems found in most Gymnosperms and Angiosperms. The filamentous condition is then wholly lost, though the system of relations and especially the polarity is maintained throughout all the changes in the apical meristem.

I feel confirmed in regarding the construction of the sporophyte in this fashion by the fact that it fits naturally with the conclusions resulting from the masterly comparative treatment of the embryology of the vascular cryptogams by Professor Bower. These are (1) the primary importance of the longitudinal axis of the shoot, the position of the first root and the foot being variable; (2) the constancy of the position of the stem-apex near the center of the epibasal half of the embryo; (3) the probability that embryos without suspensors have been derived from forms with suspensors, without any example of the converse change. These and other related facts seem to find their morphological explanation in the shoot of the sporophyte being the result of the elaboration of a filament.

The view to which we are thus led is that the uniaxial shoot is a complex whole, equivalent to the axial filament together with its congenitally associated subordinated "branches." This applies to the multicellular plant-bodies found in various independent lines of algæ and Bryophyta, whether they have definite projecting appendages of the nature of leaves or not. The discarding of the distinction between thallus and shoot, which in practise has proved an unsatisfactory one, is no great loss. Even taking the word in the narrower sense of a stem with distinct leaves, the shoots in algæ, liverworts, and mosses, though admittedly independent developments, exhibit an essential correspondence amounting to a homology of organization. The resemblances are not analogies, for it is doubtful whether the "leaves" in the different cases correspond in function. The comparison of the shoot of the sporophyte of a vascular cryptogam with, for example, the shoot of the moss seems equally justifiable. It is only forbidden by strict phyletic morphology, which for our purpose has no jurisdiction. The general agreement as regards the leaf-arrangement between the ferns and the Bryophyta suggests that similar laws will be found to hold in the shoot of both gametophyte and sporophyte. Apart from plagiotropic

shoots, there is a constructionally dorsiventral type of fern-rhizome. The leaves of this alternate as in the leafy liverworts, while the radial type of fern corresponds to the moss-shoot. It is significant that the early leaves of radially constructed ferns usually exhibit a divergence of $\frac{1}{3}$ in the seedling, passing higher up the stem into more complicated arrangements, and the same is the case in mosses. I must not enter into questions of phyllotaxy, but may remark on the hopefulness of attacking it from the study of the simpler shoots of algæ and Bryophyta rather than, as has usually been done, beginning with the flowering plants.

In some ferns (the striking example being *Ceratopteris*) the relation between the segmentation of the apical cell and leaf-production is as definite as in the moss, each segment giving rise to a leaf. This may hold more widely for ferns than is at present demonstrated, and the question deserves thorough reinvestigation to ascertain the facts independently of any theoretical views. That the coincidence of the segmentation of the shoot expressed by the leaf-arrangement and the segmentation of an apical cell is not a necessary one is, however, clearly shown in other ferns, and is obvious in the case of shoots with a small-celled meristem. The two segmentations appear to be determined by some deeper system of relations, which may also be manifested in a cœnocytic plant-body.

In the complication of the uniaxial shoot introduced by branching also there seems to be an advantage in a wide area of comparison. The question most often discussed concerns dichotomous and monopodial branching. If the details of development are to be taken into consideration, the term "dichotomy" has usually been very loosely applied. Apparent dichotomy, the continuation of one shoot by two equally strong ones, is fairly common. But in most cases investigated in detail the branching seems to be really monopodial and the forking due to the equally strong development of a lateral branch close to the main apex, not to the division of the latter. In plants growing by a single initial cell almost the only case of strict dichotomy known is the classic one of Dictyota. The branching of the ferns has been the subject of numerous investigations, but there is a great lack of developmental data. Usually the branches stand in some definite relation to the leaves of the shoot, behind, to one side, or on the leaf-base, itself, the most interesting but least common case being when the branch is in an axillary position. When the mature shoot only is considered, it is possible to argue for the derivation of monopodial branching from dichotomy or the converse. Even the facts obtainable from the mature plant, however, point to the dichotomous branching being a modification of the monopodial, the opposite view appearing to land us in difficulties regarding the morphology of the main shoot. It is unlikely that a dichotomy involving the division of the apical cell occurs in the fern-shoot, and comparison with the Bryophyta confirms the suspicion that the cases of dichotomy are only apparent.

In considering the construction of the shoot we are at present limited to comparison of the normal structure and development. The system of relations in the shoot of the fern, affecting in the first place the distribution of the leaves and secondly that of the branches, appears, however, to be of the same nature as in the independently evolved shoots of Bryophyta and algæ. A morphological analysis based on the simpler examples may lead on to the experimental investigation of the com-

mon construction. The relation that exists between the general construction and the vascular anatomy offers a special and more immediately hopeful problem. Here also, in considering the fern, we are assisted by homologies of organization in other vascular cryptogams and in the more complex Bryophyta, though the algæ are of little help.

In few departments of botany has our knowledge increased so greatly and become so accurate as in that of vascular anatomy. The definiteness of the structures concerned and the fact that they have been almost as readily studied in fossil as in living plants has led to this. Not less important have been the clear concepts first of the bundle system and later of the stele under which the wealth of fact has been brought. Great progress has been made under the influence of phyletic morphology, and anatomy has adopted further conventions of its own and tended to treat the vascular system as if it had an almost independent existence in the plant. The chief method employed has been the comparative study of the mature regions, of necessity in the fossils and by choice in the case of existing plants. I do not, of course, mean to say that we are ignorant of the development of the vascular system, but the variety in it has not been adequately studied in the light of apical development. A gap in our knowledge usually comes between the apical meristem itself and the region with a developed vascular system. It is in this intermediate region that the real differentiation takes place, and the arrangement of the first vascular tracts is then modified by unequal extension of the various parts. The apical differentiation requires separate study for each grade of complexity of the vascular system, even in the same plant.

If we look at the vascular system, not as if it had an independent existence or from the phyletic point of view, but as a differentiation taking place within the body of the individual plant, we can inquire as to the causal factors in the process. A deeper insight into the nature of the stele may be obtained by regarding it as the resultant of a number of factors, as part of the manifestation of the system of relations in development. The first step towards this is the critical consideration of normal developing plants, but so long as the causal influences in the developing substance of a plant remain unchanged the resulting vascular structure will remain constant. Our hope of advance lies in the study of cases where these influences are modified. Herein lies the value of abnormalities, of natural experiments, and the results of experimental interference. Possible influences that have at various times been suggested are functional stimuli, the inductive influence of the older pre-formed parts on the developing region, and formative stimuli of unknown nature proceeding from the developing region. The functional stimuli do not come into play at the time of laying down the vascular tracts, though they may have importance in their maintenance later; the inductive influence of the anatomy of older regions is excluded in the first differentiation of the vascular system in an embryo; we are thus led to attach special importance to the detection of the action of formative stimuli proceeding from the young developing primordia. We have further to take external stimuli into account, though these must act by influencing the internal system of relations.

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I have touched on a number of large questions, any one of which demanded separate treatment. My concern has not, however, been with them individually, but as cognate problems justifying the deliberate

adoption of a causal explanation as the aim of morphological work. I have confined myself to problems bearing on the development and self-construction of the individual, and tried to treat them so as to illustrate the causal attitude and possible lines of attack. Preliminary speculations on the questions considered can at best contain a germ of truth, and must be subsequently adjusted in the light of further facts. I have discussed these questions rather than the smaller modifications in individual development shown in metamorphosis, partly because the latter have of late years been treated from a causal point of view, and partly because I wished to consider questions that immediately affect us as working morphologists.

Did time allow, we should naturally be led to recognize the same change of attitude in biological science toward the problems of the origin of new forms. Questions of bud-variation and mutation are clearly akin to some of those considered, and the whole subject of genetics is a special attempt at a causal explanation of form and structure and the resulting functions. Close cooperation between the morphological analysis of the plant and the genetic analysis attained by the study of hybridization is most desirable. It is especially desirable that both should deal with structure as well as with form, and in the light of individual development.

The causal factors which have determined and guided evolution can be naturally regarded as an extension of the same line of inquiry. The Darwinian theory, and especially the exposition of the principle of natural selection, was the greatest contribution ever made to the causal explanation of the organic world. Strangely enough, it led to a period of morphological work in which the causal aim was almost lost sight of. Why evolution has taken place in certain directions and not in others is a problem to the solution of which causal morphology will contribute. The probability of orthogenesis, both in the animal and vegetable kingdoms, is again coming into prominence, however it is to be explained. When we consider the renewed activity in this field it is well to remember that, just as is the case with causal morphological work, we are picking up a broken thread in the botanical web. Lastly, as if summing up all our difficulties in one, we have the problem of adaptation. In attacking it we must realize that use and purpose have often been assumed rather than proved. We may look to scientific ecological work to help us to estimate the usefulness or the selection value of various characters of the plant. On the other hand, causal morphology may throw light on whether the "adaptation" has not, in some cases at least, arisen before there was a "use" for it. The hopeful sign in the recent study of these greater morphological problems is that the difficulties are being more intensely realized, and that rapid solutions are justly suspect. The more the causal attitude is adopted in ordinary morphological work, the more hope there is of these larger questions being inductively studied rather than argued about.

The causal aim is essentially different from the historical one, but there is no opposition between causal and phyletic morphology. They are rather mutually helpful, for there has been an evolution, not of mature plants, but of specific substances exhibiting development. A deeper insight into the nature of ontogeny is thus bound to be of assistance to phyletic morphology, while the tested results of phyletic work afford most valuable guidance in general causal morphology, though this can not accept any limitation to single lines of descent in its comparisons.

I have tried to bring before you the possibilities of causal morphology partly because the same attention has never been given to it in this country as to other branches of botany, and partly because if morphology be conceived in this broader spirit it need not be said that it has no practical bearing. I should not regard it as a serious disability were the study of purely scientific interest only, but this is not the case. When, if ever, we penetrate into the secrets of organization so far as to be able to modify the organism at will (and genetics has advanced in this direction), the practical possibilities become incalculable.

Probably all of us have reflected on what changes the war may bring to botanical work. It is impossible to forecast this, but I should like to emphasize what my predecessor said in his address last year as to pure science being the root from which applied science must spring. Though results may seem far off, we must not slacken, but redouble our efforts towards the solution of the fundamental problems of the organism. This can be done without any antagonism between pure and applied botany; indeed, there is every advantage in conducting investigations on plants of economic importance. It would be well if every botanist made himself really familiar with some limited portion of applied botany, so as to be able to give useful assistance and advice at need. The stimulus to investigation would amply repay the time required. Even in continuing to devote ourselves to pure botany we can not afford to waste time and energy in purposeless work. It is written in "Alice in Wonderland" that "no wise fish goes anywhere without a porpoise," and this might hang as a text in every research laboratory.

A plant is a very mysterious and wonderful thing, and our business as botanists is to try to understand and explain it as a whole and to avoid being bound by any conventional views of the moment. We have to think of the plant as at once a physico-chemical mechanism and as a living being; to avoid either treating it as something essentially different from non-living matter or forcibly explaining it by the physics and chemistry of to-day. It is an advantage of the study of causal morphology that it requires us to keep the line between these two crudities, a line that may some day lead us to a causal explanation of the developing plant and the beginnings of a single science of botany.

W. H. LANG

UNIVERSITY OF MANCHESTER

WHICH OF THE PRESENT MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE HAVE HELD THE LONGEST CONTINUOUS MEMBERSHIP?

THIS is a question which often comes up at the meetings and several of the old-time members occasionally claim to be the oldest living member.

Mr. F. S. Hazard, the assistant secretary of the association, in going over the list of members, has drawn up the following statement concerning the members now living:

*Hilgard, Eugene Woldemar, Ph.D., LL.D., University of California, Berkeley, Calif. (11.) 1874. B, C, E.

*Würtele, Rev. Louis Campbell, P. O. Drawer E, Acton Vale, Quebec, Canada. (11.) 1875. E, H.

*Paine, Cyrus Fay, 520 East Avenue, Rochester, N. Y. (12.) 1874. A, B.

*Fairbanks, Rev. Henry, Ph.D., St. Johnsbury, Vt. (14.) 1874. A, B, D.

*Wright, Arthur Williams, Ph.D., Yale University, New Haven, Conn. (14.) 1874. A, B.

*Raymond, Rossiter W., Ph.D., LL.D., 29 West 39th St., New York, N. Y. (15.) 1875. B, C, D, E, I, K.

*Abbe, Cleveland, Ph.D., LL.D., U. S. Weather Bureau, Washington, D. C. (16.) 1874. A, B.

*Beal, William James, Ph.D., Sc.D., 40 Sunset Avenue, Amherst, Mass. (17.) 1880. G.

*Clark, John Emory, 34 South Park Terrace, Long Meadow, Mass. (17.) 1875. A.

*Hinrichs, Gustavus D., M.D., 4106 Shenandoah Avenue, St. Louis, Mo. (17.) 1874. B, C.

*Perkins, George Henry, Ph.D., LL.D., University of Vermont, Burlington, Vt. (17.) 1882. E, H.

*Tuttle, Albert Henry, professor of biology, University of Virginia, Charlottesville, Va. (17.) 1874. F.

*Ward, Richard Halsted, M.D., 53 Fourth St. Troy, N. Y. (17.) 1874. G.

*Bethune, Charles James Stewart, professor of entomology, Ontario Agricultural College, Guelph, Canada. (18.) 1875. F.

*Clarke, Frank Wigglesworth, Sc.D., LL.D., U. S. Geological Survey, Washington, D. C. (18.) 1874. C.

*Dall, William Healey, Sc.D., Smithsonian Institution, Washington, D. C. (18.) 1874. F, G.

*Goodale, George Lincoln, M.D., LL.D., Harvard University, Cambridge, Mass. (18.) 1875. G. (Past president.)

*Morley, Edward Williams, Ph.D., Sc.D., West Hartford, Conn. (18.) 1876. B, C, E. (Past president.)

*Pickering, Edward Charles, Ph.D., Sc.D., LL.D., Astronomical Observatory, Harvard University, Cambridge, Mass. (18.) 1875. A, B. (Past president.)

*Rice, William North, Ph.D., LL.D., Wesleyan University, Middletown, Conn. (18.) 1874. E, F, L.

*Stockwell, John Nelson, Ph.D., 2302 Murray Hill Road, Cleveland, Ohio. (18.) 1875. A, B, C.

*Williams, Henry Shaler, Ph.D., Cornell University, Ithaca, N. Y. (18.) 1882. E.

*Chandler, Charles Frederick, Ph.D., M.D., Sc.D., LL.D., professor of chemistry, Columbia University, New York, N. Y. (19.) 1875. C.

*Cooley, LeRoy Clark, Ph.D., 2 Reservoir Square, Poughkeepsie, N. Y. (19.) 1880. B, C.

*Emerson, Benjamin Kendall, Ph.D., P. O. Box 203, Amherst, Mass. (19.) 1877. E.

*Hale, William Henry, Ph.D., 40 First Place, Brooklyn, N. Y. (19.) 1874. A, B, C, E, F, H, I.

Peck, William Adorno, C.E., 1643 Champa St., Denver, Colo. (19.) D.

Clark, Miss Anna M., 400 West 118th St., New York, N. Y. (20.) H.

*Farlow, William Gilson, Ph.D., M.D., LL.D., professor of cryptogamic botany, Harvard University, Cambridge, Mass. (20.) 1875. G.

*Garman, Samuel, Museum of Comparative Zoology, Harvard University, Cambridge, Mass. (20.) 1874. E, F.

*Smith, Eugene Allen, Ph.D., LL.D., state geologist, University, Alabama. (20.) 1877. C, E.

(The asterisk denotes that the member is a Fellow; the number in parenthesis indicates the meeting at which the member joined the association; the date following is the year when made a Fellow; the letters at the end of line indicate the Sections to which the member or Fellow belongs.)

It is appropriate to mention that the late permanent secretary, Frederic Ward Putnam, emeritus professor of anthropology, Harvard University, Cambridge, Mass., who died only last summer, was, at the time of his death, the oldest living member of the association, having joined at the tenth meeting which was held in Albany in 1856.

L. O. Howard,
Permanent Secretary

THE MARINE BIOLOGICAL LABORATORY

The attendance at the Marine Biological Laboratory, Woods Hole, has been as follows:

	1911	1912	1913	1914	1915
Investigators — total	82	93	122	129	137
Independent:					
Zoology	42	44	58	62	69
Physiology	18	14	17	22	20
Botany	8	10	11	10	6
Under instruction:					
Zoology	12	21	21	31	36
Physiology	2	2	7	1	4
Botany		2	7	3	2
Students — total	65	67	69	89	105
Zoology	26	24	33	43	47
Embryology	20	15	22	21	37
Physiology	6	11	8	10	15
Botany	13	17	7	15	6
Total attendance	147	160	191	218	242
Institutions represented — total		57	80	77	79
By investigators	37	43	50	51	59
By students	31	36	41	47	42
Schools and academies represented					
By investigators	3	2	3	1	3
By students	9	1	6	5	9

The evening lectures during the season were:

July 2, "Experimental Production of Racial Degeneracy by Alcohol Poisoning," by Professor C. R. Stockard.

July 6, "The Seals of the Pribiloff Islands," by Professor G. H. Parker.

July 9, "Some Experimental Studies on the Development of the Membranous Labyrinth in the Tadpole," by Professor G. L. Streeter.

July 13, "Effects of Centrifugal Force on the Structure and Development of the Egg," by Professor E. G. Conklin.

July 16, "The Nature of Intelligent and Purposive Action from a Physiological Point of View," by Professor R. S. Lillie.

July 19, "The Control of Infection as Affected by Variation among Parasitic Microorganisms," by Professor Simon Flexner.

July 24, "Protozoa and the Cancer Problem," by Professor G. N. Calkins.

July 30, "Inheritance of Sex in *Lychnis*," by Professor George Shull.

August 3, "Heredity of Criminality," by Professor C. B. Davenport.

August 6, "The Story of Bubonic Plague," by Dr. Martin Edwards.

August 10, "The Rôle of Adsorption in Nerve Conduction," by Dr. Alfred G. Mayer.

SCIENTIFIC NOTES AND NEWS

AT the request of President Wilson the National Academy of Sciences has appointed the following to investigate and report on the control of the Panama slides: President C. R. Van Hise, University of Wisconsin, chairman; General H. L. Abbott, Cambridge, Mass.; Dr. George F. Becker, United States Geological Survey; Professor J. C. Branner, Stanford University; Dr. Whitman Cross, United States Geological Survey; Dr. Arthur L. Day, Carnegie Institution; Dr. J. L. Hayford, Northwestern University; Professor Harry Fielding Reid, Johns Hopkins University; Dr. R. S. Woodward, Carnegie Institution; C. Carpenter, Ithaca, N. Y.; August P. Davis, United States Reclamation Service; John R. Freeman, Providence, R. I.

PROFESSOR WILLIAM B. SCOTT, of Princeton, and Professor A. A. Michelson, of Chicago, have been appointed representatives from the American Philosophical Society to the Second Pan-American Congress meeting in Washington, December 27 to January 8.

DR. ROBERT BARANY, of the University of Vienna, who has been awarded the Nobel prize in medicine for his work on the diseases of the ear, is at present a prisoner of war in Siberia, having been serving as a surgeon at Przemysl when it was taken by the Russians.

DR. R. W. WOOD, professor of physics at Johns Hopkins University, has returned to Baltimore from the Mount Wilson Observatory, where he has been engaged in photographing the moon, Saturn and Jupiter by yellow light and ultra violet and infra red rays.

AT a recent meeting of the executive committee of the American Museum of Natural History, New York, the new position of research associate was created on the scientific staff of the institution. Dr. C. R. Eastman was appointed research associate in vertebrate paleontology, and Mr. M. C. D. Crawford, research associate in textiles.

PROFESSOR OTTO BASCHIN, of Berlin, has received from the Royal Prussian Academy of Sciences the silver Leibnitz medal in recognition of his services to geography.

AT the annual meeting of the Royal Faculty of Physicians and Surgeons of Glasgow, Dr. Ebenezer Duncan was elected president, and the retiring president, Dr. John Barlow, a councillor.

MR. E. D. MERRILL, who has been spending several months in Washington, D. C., returns to Manila in December to resume his duties as botanist at the Bureau of Science and professor of botany in the University of the Philippines.

THE annual meeting of the Southern Medical Association was held at Dallas, Texas, on November 9, 10 and 11. The principal address was delivered by Dr. Rupert Blue, surgeon-general of the United States Public Health Service, on "Sanitary Preparedness." Other addresses were given by Dr. W. L. Rodman, president of the American Medical Association, and Dr. Isador Dyer, of New Orleans.

ON November 23 Dr. R. P. Strong, professor of tropical medicine at Harvard University, gave a public lecture on his experiences as head of the physicians who went to Serbia to combat the epidemic of typhus fever.

The lecture was given for the benefit of the social service department of the Massachusetts General Hospital.

Dr. J. G. Needham, of Cornell University, delivered two addresses at the University of Kansas, in the third week of November: one before the Entomological Club of the University on the "Ecology of Certain Aquatic Larvæ," and the other before the students of the university on the "Common Ground of Poet and Naturalist."

Professor D. W. Johnson, of Columbia University, addressed the Yale chapter of Sigma Xi at New Haven on November 18, on the "Surface Features of Europe as a Factor in the War."

Since Mr. Donald B. Macmillan has not returned from the Arctic in time to present to the members this fall an account of the Crocker Land expedition which he led north in 1913, arrangements have been made to show to members of the museum on the evening of December 9 the motion pictures secured by Sir Douglas Mawson on the Australasian-Antarctic expedition. A brief account of the expedition and a description of the action of the films, will be given by Mr. George H. Sherwood, curator of education at the American Museum of Natural History.

The Swiney lectures on geology are being delivered this year by Dr. J. D. Falconer, the subject being "Ice and the Ice Age." The courses are given at the Victoria and Albert Museum, London.

At the age of eighty-five years Sir Allen William Young, the Arctic explorer, died in London, on November 27.

Professor James McCall, principal of the Glasgow Veterinary College, died on November 1, at the age of eighty-one years.

Lieut. Gordon Sanderson, known for his work on the Archeological Survey of India, was killed in France on October 13.

Mr. Donald Ewen, known for his work in metallurgy, has been killed in the war. *Nature* states that he was serving at the time as a private in the London Scottish Regiment, but an order had been issued for his recall, to take up important work at the National Physical Laboratory.

Dr. Gaston Vasseur, professor of geology in the University of Marseilles, has died at the age of sixty years.

Professor Sklarek, for many years editor of the *Naturwissenschaftliche Rundschau*, died in Berlin, on October 10, at the age of eighty years.

The annual sessions of the Federation of American Societies for Experimental Biology, formed by the Physiological Society, the Biochemical Society, the Pharmacological Society and the Society for Experimental Pathology, will be held at the Harvard Medical School in Boston on December 27, 28 and 29.

The following program has been arranged for the meeting of the Sigma Xi to be held in connection with the meeting of the American Association for the Advancement of Science at Columbus: Tuesday, December 28, at 12 o'clock, meeting of the executive committee at lunch at the Ohio Union. At 4 o'clock at Page Hall, annual convention. At 6:30 at Ohio Union, annual dinner.

In connection with the Congress of Clinical Surgeons, meeting in Washington, November 26–27, an interesting exhibit of books and pictures illustrating the history of surgery has been prepared by Dr. Arnold C. Klebs at the invitation of Colonel C. C. McCulloch, librarian of the Surgeon General's Office, and may now be seen in the library hall of the Army Medical Museum.

Adelphi College, Brooklyn, New York, has just been the recipient of an outfit for instruction in astronomy from a friend whose name is withheld. It consists of a telescope of six and a half inches aperture, with driving clock and twelve inches declination and right ascension circles, a six-prism spectroscope which was made to order by Browning, a small transit by Steger of Kiel, and other accessory apparatus. The telescope was made by Alvan Clark, in 1877, ten years before his death, and is considered an excellent instrument. The apparatus is in perfect condition, having had the best of care by its former owner.

AMONG additions to the American Museum of Natural History is a model showing the increased efficiency of the present hospital service at Panama over that of the French period. This model shows a hospital in the French period in care of a Sister of Mercy. Puddles of water were allowed to gather about the ground, and the legs of the beds were placed in cans of water to prevent ants from crawling up. As we now know, yellow fever and malarial fever mosquitoes bred in such accumulations of stagnant water and helped to keep the hospitals well filled. Screens were not used and the ventilation was not of the best. The companion part of the model shows a French hospital, altered to conform to our most modern ideas and knowledge of the relation of insect and disease. A clean, dry cellar, well-kept grounds, screens, increased ventilation and the care of trained nurses serve to change an insanitary, disease-breeding building into the acme of sanitation.

THE Cornell University Medical College has recently reorganized its surgical service at Bellevue Hospital. At the head of the service there is now one "visiting surgeon in charge" with a continuous service. He has general supervision over the entire work and is responsible only to the college and the hospital for its proper performance. There are two visiting surgeons who also have a continuous service limited to some special subdivision of general surgery. They are thus relieved of all routine work in order to devote their time to the particular work with which they are occupied. Under the visiting surgeon in charge are two associate visiting surgeons. These men are on the full time salaried basis, and each has the care of one half of the service. They have as assistants four juniors who are also surgeons to the out-patient department. The organization now also includes a laboratory of surgical pathology and a laboratory of experimental surgery, each under a full-time salaried man. These laboratories are available to all members of the staff, who there have the privilege of working on their individual problems. The entire staff has been appointed to positions in the department of surgery in the Medical College, and all students take a portion of their surgical ward work under this organization. The essential changes from the former system consist in having one head, continuous service, full-time salaried surgeons and laboratories under the immediate jurisdiction of the surgical service. Following is the staff as at present constituted: John A. Hartwell, M.D., assistant professor of surgery, visiting surgeon in charge; George Woolsey, M.D., professor of clinical surgery, visiting surgeon; John Rogers, M.D., professor of clinical surgery, visiting surgeon; Kenneth Bulkley, M.D., instructor in clinical surgery, associate surgeon; James Worcester, M.D., instructor in clinical surgery, associate surgeon; Fenwick Beekman, M.D., instructor in operative surgery, junior surgeon; Benjamin Vance, M.D., instructor in pathology, surgical pathologist; J. W. McMeans, M.D., assistant in surgery, assistant in experimental surgery.

UNIVERSITY AND EDUCATIONAL NEWS

OBJECTIONS have been filed to the probating of the will of Amos F. Eno who bequeathed a large sum to public purposes and made Columbia University his residuary legatee. It is said that under the will Columbia University would receive $3,000,000 or more.

A BEQUEST of $50,000 has been made to Cornell University by Mrs. Sarah Manning Sage to promote the advancement of medical science by the prosecution of research at Ithaca.

PROFESSOR H. L. BOWMAN, Waynflete professor of mineralogy at Oxford, reports, according to *Nature,* two gifts to his department. (1) Under the will of the late Sir Arthur Church has been bequeathed £100 for the purchase of apparatus and specimens, together with the chemical and mineralogical apparatus and instruments in Sir Arthur's laboratory and his collection of mineral specimens (other than cut gem-stones). (2) A collection of minerals made by the late Dr. Hugo Müller, containing some 2,000 specimens, has been presented by Mrs. Müller.

DR. ERNEST FOX NICHOLS has resigned the presidency of Dartmouth College and will go to Yale University next year to fill a new chair

of physics in the academic department. Dr. Nichols will at the close of the present academic year have served as president of Dartmouth College for seven years, having previously been professor of physics at Colgate, Dartmouth and Columbia. In accepting the resignation the trustees of Dartmouth College write:

> It had been our hope that Dartmouth College might long continue to enjoy your leadership. Yet we can but recognize that the sacrifices which you have already made deserve worthier recognition than the demand that you continue them at serious cost to your own well-being. In the chosen field of science to which you are about to return you will carry our sure expectation of great accomplishment and added honors; but more especially you will carry our warm personal affection, the outgrowth of seven years of intimate fellowship in a common cause.

Professor T. W. Galloway, Ph.D., who has occupied the chair of biology at James Millikin University at Decatur, Ill., since the establishment of that institution in 1903, has been appointed professor of zoology at Beloit College, Beloit, Wisconsin. A. A. Tyler, Ph.D. (Columbia, '97), for some years professor of biology in Bellevue College, Omaha, Nebraska, has been appointed to the chair of biology at James Millikin University, to succeed Dr. Galloway.

At Harvard University James Sturgis Pray, of Cambridge, has been elected as Charles Eliot professor of landscape architecture, succeeding Frederick L. Olmsted, resigned and Robert W. Lovett, of Boston, as Brown professor of orthopedic surgery.

DISCUSSION AND CORRESPONDENCE

GENUS AND SUBGENUS

To the Editor of Science: I have read with interest the discussion of the genus in taxonomy which has been running in recent numbers of Science. I am especially interested in Dr. Allen's condemnation as "intolerable" of "the use of both the generic name in the broader sense, and the subgeneric name (in parenthesis) in incidental references." Emphasizing the last three words of the quotation, one may endorse Dr. Allen's condemnation. But I believe the practise of retaining old genera, except in cases in which they express false concepts of relationship, is often a good one, and that newly discerned natural groups of species within the old genus may better be treated as subgenera.

I have recently reviewed the well-known genus *Salpa* and have had to recognize ten subdivisions in order to express the major groups before coming to species distinctions. It seemed a pity to discard the old genus name *Salpa*. I therefore retained this and classed the ten subdivisions as subgenera, though, if one wished to do so, he could thoroughly justify them as genera. The special student of the *Salpidæ* will bear in mind the subgeneric names and very likely will use them in highly special papers, *e. g., Thalia democratica, Ritteria retracta, Apsteinia punctata,* etc. But in general reference all or any of these would be *Salpa*.

We must recognize numerous supra-specific subdivisions of many old genera and these must be named, but let the broader old generic name be the one in use except when one desires to call attention to the diversities emphasized by the subgeneric names. In the latter case, at the risk of Dr. Allen's condemnation, I would use parenthetically the subgeneric name also. This is a bit awkward, but such minutely distinctive terminology is not so frequently needed. Using the broader generic name merely refuses to introduce unnecessary reference to subgeneric classification. When this is germane to the discussion, of course introduce it. But let us not insist on always dragging in the whole subject in all its intricacies when by so doing we merely distract attention from what we are saying.

In ordinary reference to squirrels it is sufficient to call them *Sciurus,* and the fact that this name so used includes "a score or more of natural groups sharply defined geographically and by minor but not unimportant morphological characters" does not present any argument against such terminology, provided we have at our disposal a subsidiary terminology which can be introduced when the distinc-

tions between the lesser natural groups are relevant to the subject under discussion.

It is difficult to see, in the case of *Sciurus*, or any other group, that we are any better off when we have divided all our species into numerous geographical (and other) species, and have called the old species genera, of course changing the names in the process, than we are when we retain the old species, calling their now recognized subdivisions subspecies, races and forms, and treat as subgenera rather than genera newly discerned natural groups of species within the old genus. Of course, an old genus, if shown to be unsound and to express a false concept of relationship, will be abandoned.

There is almost no limit to the niceties of taxonomic analysis that might be introduced by breeding of all animal species. Any classification short of one founded on such complete data is conventional. The practical question is, what convention shall we adopt? The one here advocated retains, in so far as they are valid, old genus and species names, using a subsidiary nomenclature of subgenera, subspecies, etc., for the more intimate distinctions.

There are several advantages in this course. It does not change general conceptions of genus and species to something of a different grade of taxonomic value. It keeps us in touch with the zoology of the past (*i. e.*, that of year before last). It saves immense labor in ascertaining what forms are meant by the unfamiliar names when one is reading outside his special field. It ensures more general understanding by one's readers. It does not limit completeness of taxonomic analysis, which is recorded in the subsidiary nomenclature. It confines to the field of the specialist, who uses the subsidiary nomenclature only when writing for his fellow specialist, most of the confusion which comes from the acceptance and later the rejection of unjustified terminology. It thus saves the general literature of zoology from the introduction of an immense deal of confusion.

MAYNARD M. METCALF
THE ORCHARD LABORATORY,
OBERLIN, OHIO,
October 26, 1915

NOTES ON THE PERMO-CARBONIFEROUS GENUS CRICOTUS COPE

IN Publication No. 207 of the Carnegie Institution of Washington I described and figured an ilium from the Brier Creek Bone Bed of the Wichita Formation in Archer Co., Texas (page 161, pl. 22, figs. 2 and 3). This bone was assigned to the genus *Cricotus* because of the relative abundance of the specimens associated with large numbers of the vertebral and intervertebral centra of that genus. The peculiar form of the ilium, unique and previously unknown from the North American beds, makes it of peculiar value in correlating faunas of widely separated localities. In looking over Fritsch's "Fauna der Gaskohle" I find an almost identical form of this bone described and figured for two genera, *Diplovertebron*[1] and *Macromerion*.[2] Fritsch recognized these elements as ilia but in some figures confuses parts of the bones with the ischia and pubes. It is at once obvious from a comparison of his figures with those published by me that the bones from the two widely separated localities are nearly identical, even to the smallest details. Unfortunately the ischia and pubes of the Bohemian forms were only partly known to Fritsch and he publishes figures of fragments only.

The two Bohemian genera, from the upper Carboniferous, are embolomerous forms and many of the bones figured by Fritsch as associated with the ilia are strikingly like those assigned to *Cricotus* from the Brier Creek Bone Bed, notably the femur and the smaller bones of the limbs. The inter-centra of *Macromerion schwarzenbergii*[3] are indistinguishable from those of *Cricotus*. The teeth also show many resemblances in the two types, especially in the manner of fixation to the jaw and the slightly infolded dentine of the base.

There can remain no doubt that the family Cricotidæ was present in Bohemia and North America at nearly the same time and was represented by closely related genera. This adds

[1] Bd. 11, Taf. 52, Fig. 2 and Taf. 53, Fig. 14.

[2] Bd. 11, Taf. 6, Figs. 1, 2; Taf. 67, Figs. 1, 2; Taf. 69, Fig. 1.

[3] Taf. 66, Figs. 5*a*, *b*, *c*.

materially to the evidence that the deposits in the two regions were cotemporaneous, a fact hitherto suggested only by the common occurrence of the genus *Edaphosaurus* (*Naosaurus*).

It has been shown by me[4] that North America was probably isolated from the Old World in Permo-Carboniferous time, at least for such forms as the Amphibia, and the suggestion arises of the great antiquity of the embolomerous type permitting such a distribution, a suggestion borne out by Moodie's find of an embolomerous form, *Spondylerpeton spinatum,* in the Mazon Creek beds of Illinois.

Further work suggested by the facts here stated is in progress.

E. C. Case

A SIMPLE METHOD OF INDICATING GEOGRAPHICAL DISTRIBUTION

In a recent number of Science a method of showing geographical distribution is suggested.[1] All who have to work with these problems will agree that political boundaries are unsatisfactory in such work, and also that the system of geographic coordinates (parallels and meridians) is often too exact for the information in hand, and, moreover, does not give a very clear idea of the location to most readers. Although there are obvious disadvantages in the use of rectangular areas such as those suggested, it is probable that their advantages are even greater.

A modification of the boundaries suggested seems desirable from the point of view of uniformity among the sciences. After thorough discussion at several international geographic congresses the government surveys have undertaken the preparation of an international map of the world on a scale of 1 to 1,000,000. The quadrangle adopted for this map seems nearly if not quite as well suited for showing distribution as that suggested recently. If this quadrangle can be adopted there will be a single system of areas for the topographic map of the world and for the purpose of stating distribution, and this has the great advantage of simplicity. There is the further advantage that the statement of the location in the new system will show directly what topographic sheets will give the actual physical environment of the species under discussion.

The quadrangle of the international map is 4 degrees of latitude by 6 degrees of longitude; these quadrangles are designated by a system of letters beginning at the equator and numbers beginning at longitude 180°. The surface of the earth is divided into zones bounded by parallels of latitude, each zone is 4 degrees wide and extends around the earth. Zone *A* extends from the equator to latitude 4°, zone *B* from latitude 4° to latitude 8°, and so on; there are separate sets of zones north and south of the equator, that north of the equator designated by the word "north" and that south of the equator by the word "south." There are also north polar and south polar sheets, each circular and 4 degrees in diameter. The quadrangles of each zone are numbered from longitude 180° eastward around the earth. Thus the two sheets of the international map already published for the United States are *Boston,* North K 19 (latitude 40° to 44° N., longitude 72° to 78° W.) and *San Francisco,* North J 10 (latitude 36° to 40° N., longitude 120° to 126° W.).

The only disadvantage of the international map quadrangle, when compared with the "merospheres" suggested by Adams, is their somewhat smaller size. This is slight when compared with the gain in uniformity secured by the use of the quadrangle already adopted for mapping the world. It is to be hoped that any system of dividing the surface of the earth into quadrangles will in the future be based on the international map.

William G. Reed
Office of Farm Management,
Washington, D. C.,
September 22, 1915

NEW JERSEY CETACEA

Apropos of Mr. Fowler's note in the August 13, 1915, issue of Science, I wish to add another New Jersey record for the dolphin, *Delphinus delphis;* early in May of this year I found a dead specimen on the beach at Sea

[4] Publication 207, Carnegie Institution.

[1] Adams, J., "A Simple Method of Indicating Geographical Distribution," Science, N. S., Vol. 42, pp. 366–68, September 17, 1915.

Isle City, in Cape May County. Owing to decomposition the bones were not obtained then, and on later visit to the locality the specimen could not be found.

In addition to the above species I have secured since 1908 the following cetaceans at or near Sea Isle City; these are now in the collections of the Academy of Natural Sciences of Philadelphia: *Globicephala brachyptera* Cope, *Kogia breviceps* DeB., *Mesoplodon densirostris* DeB., and *Tursiops truncatus* Mont.

WM. J. FOX

THE ACADEMY OF NATURAL SCIENCES
OF PHILADELPHIA

Note.—Since the above was written one of the fish-pound crews at Sea Isle City brought in on September 25, 1915, seven live specimens of *Delphinus delphis.*

THE FUR SEAL REPORT

TO THE EDITOR OF SCIENCE: At pages 41, 44 and 57 of the fur seal report of Messrs. Osgood, Preble and Parker for 1914, Senate Document No. 980, recently published, occur important statistical tables giving enumerations of the different classes of seals for 1912, 1913 and 1914, conclusions and inferences from which affect vitally the report as a whole. The source of the figures for 1912 and 1913, which could only have been obtained from the field notes and unpublished reports of the writer now in the hands of the commissioner of fisheries at Washington, is not indicated and in the paragraph of general acknowledgment at page 17 credit to former workers is limited to "printed reports."

GEORGE ARCHIBALD CLARK

STANFORD UNIVERSITY, CALIF.,
November 19, 1915

ROGER BACON AND GUNPOWDER

IN his paper "Roger Bacon and Gunpowder" contributed to the "Roger Bacon Commemoration Essays" (edited by A. G. Little, Oxford, 1914), Colonel Hime tries to prove Roger Bacon the inventor of gunpowder by the method employed to prove Francis Bacon the author of Shakespere's plays—a cipher. Since other contributors to the same volume refer favorably to this effort (Mr. A. G. Little, p. 395, calls it an "ingenious explanation" and Mr. Patterson Muir, p. 301, says that "Colonel Hime establishes a large probability" in its favor), it may be well to note some points against it quite apart from the merits of the cipher itself.

In the first place, the cipher is based upon chapters of the "Epistola de secretis operibus naturæ et de nullitate magiæ" not found in the early manuscript of that work and considered doubtful by Charles in his work on Roger Bacon. Indeed, the opening phrases of two chapters, "Transactis annis Arabum sexcentis et duobus," and "Annis Arabum 630 transactis" suggest their source.

Secondly. Roger Bacon openly alludes to gunpowder in 1267 in his "Opus Tertium" as already in common use in children's toy explosives. Therefore Colonel Hime has to date the "De secretis" at 1248, and to hold that Bacon was at that time "driven to employ cryptic methods by fear of the Inquisition" (p. 334), but that by 1267

> circumstances had totally changed in the lapse of years; the composition of gunpowder . . . had been divulged, and the first use made of the deadly mixture was for the amusement of children (p. 321).

But is there any good reason for dating the "De secretis" in 1248? Much of it sounds like a brief popular compilation from Bacon's three works of 1267–8 concocted by some one else later; compare, for instance, the first paragraph of the sixth chapter of the "De secretis" with Duhem, "Un fragment inédit de l'Opus Tertium," pp. 153–4 and Little, "Part of the Opus Tertium," 50–51. The dedication of the "De secretis" to William, Bishop of Paris, who died in 1249, occurs first in the late edition of 1618 and has not been found by Little in any manuscript.

Then the inquisition bug-a-boo is negligible. Has any one ever shown that the inquisition punished a practical invention? It was not for having invented the telescope that Galileo was persecuted. Moreover, Galileo's was an exceptional case, and it can not be shown that in the thirteenth century the church persecuted men of science. Rather, popes and prelates were their patrons.

But even if we admit that Bacon wrote the "De secretis" as we have it in 1248 and was at that time afraid of the Inquisition, the question remains: why in 1267–8, when mentioning the explosive in those works in which he made such desperate efforts to secure the pope as his patron and boasted repeatedly of his own superiority to his contemporaries, did he not claim the credit of the invention which he had set forth in cipher twenty years before? The simple answer is: it was not his invention.

One instance must be added to show how Colonel Hime misinterprets the text of the "De secretis" in his eagerness to smell powder everywhere. He writes (p. 324):

> Now, towards the end of Chap. X., Bacon speaks without disguise of charcoal under the name of the wood from which it is made, and mentions the two trees, hazel and willow, which give the best. He significantly adds that when charcoal is added to proper proportions of certain other substances, something noteworthy happens. Since, then, charcoal is one of the subjects of these two chapters, it becomes all the more probable that saltpeter forms another.

In a note Hime adds the Latin of the passage in question:

> Si vero partes virgulti coryli aut salicis multarum justa rerum serie apte ordinaveris, unionem naturalem servabunt: et hoc non tradas oblivioni, quia valet ad multa.

Let us note first that these last words do not mean, "something noteworthy happens," but "don't forget this, because it's valuable." Thus the true wording does not in the least suggest an explosion, as Colonel Hime's translation does. Secondly, the words *partes virgulti coryli aut salicis* probably do not denote charcoal but twigs or rods of hazel or willow, as they do in Bacon's account of the experiment performed by magicians with a split hazel rod. It occurs both in the "Opus Maius" (Bridges, II., 219) and "Opus Tertium" (Little, 49–50; Duhem, 153); I quote the latter.

> Unde magici accipiunt virgas coruli et salicum, et dividunt eas secundum longitudinem, et faciunt eas distare secundum quantitatem palmae, et addunt carmina sua, et coniungunt partes divise; sed non propter carmina, sed ex naturali proprietate. (Wherefore magicians take rods of hazel and willow, and divide them lengthwise, and hold them the breadth of a palm apart, and add their charms, and the divided parts come together; but not on account of the charms, but from their very natures).

Thirdly, it is probably precisely this hazel-rod experiment to which the writer of the passage quoted by Hime refers. *Multarum justa rerum serie ordinaveris* seems a hurried equivalent for the more specific directions in the passages in the *Opus Maius* and *Opus Tertium*, and this bears out what I have already suggested, that the *De secretis* may be in part at least a brief popular compilation from Bacon's other works. Finally, the phrase *unionem naturalem servabunt* applies better to the bending together in the middle of two halves of a split hazel rod held apart at the ends than it does to a mixture of saltpeter, charcoal and sulphur.

And now what becomes of Colonel Hime's assertion, "Since therefore charcoal is one of the subjects of these two chapters, it becomes all the more probable that saltpeter forms another"? We may alter it to read thus: since charcoal is not a subject of either of these chapters, it becomes all the more improbable that a method of refining saltpeter is disclosed in them in cipher.

Lynn Thorndike
Western Reserve University

SCIENTIFIC BOOKS

A Meteorological Treatise on the Circulation and Radiation in the Atmospheres of the Earth and of the Sun. By Frank H. Bigelow, M.A., L.H.D., Professor of Meteorology in the U. S. Weather Bureau, 1891–1910, and in the Argentine Meteorological Office since 1910. New York, John Wiley and Sons, Inc., 1915. Pp. xi + 431. 78 figures in the text.

This volume is an elaboration of the papers on atmospheric thermodynamics which Professor Bigelow published in the *American Journal of Science* for December, 1912, and March, 1913, with additions on the laws of storms, on solar constant of radiation, on atmospheric

electricity, methods of barometric reduction, the meteorology of the isothermal layer, on the laws of evaporation, and a few other subjects to which the author has paid special attention for many years by methods which are often highly original. The book may be regarded as a summation of its author's labors on meteorological theory, and records not a few notable advances in meteorological procedure, especially in regard to the reduction of observations.

To the author of this treatise we already owe the standard system of barometry now used by the Weather Bureau of the United States, an important concatenation of terrestrial meteorological phenomena and solar changes which is here presented anew in slightly revised form, a considerable modification of Ferrel's equations for atmospheric movement in storms with an application to tornadoes which has given, for the first time, reliable data for these very intense movements of the air, and some interesting and highly original, though perhaps not entirely conclusive speculations in regard to the causes of the several variations in the electric and magnetic elements. The present work records a further advance in measures of the ionization of the atmosphere which enable the author to clear up the discrepancy of about 300 per cent. which has hitherto existed between different methods of measuring the dissipation of electricity in the atmosphere.

The special topic around which all the others cluster is the application of the universally accepted thermodynamic equations to atmospheric phenomena. In order to accomplish this, the author is obliged to make the gas coefficient (R) of the usual thermodynamic equations a variable, instead of a constant. It may seem a little risky thus to throw wide open the doors of theory, and to make this fundamental "constant," so-called (whose value has been accurately determined from laboratory experiments) vary through a wide range, while still retaining the form of the equations whose accuracy rests upon these same laboratory experiments. We may ask whether the equations whose accuracy as mathematics is conceded, will continue to be applicable if their basis is changed. More than one meteorologist has suggested to the reviewer that Bigelow seems to be trying to make the laws of nature conform to his equations, with the implication that success in this attempt is somewhat doubtful. It can not be denied that an element of empiricism enters into the proposed methods. The author points out correctly that the well-known departure of the atmosphere from adiabatic conditions is inevitable as long as heat and motion (of air currents) are imported into any given volume of air. This is self evident; but will the thermodynamic equations stand up under this extra burden when the effects of wind and radiation, together with several other activities not explicitly named in this "Treatise," are included? The author claims that everything checks, but on examination this is found to mean simply that when limited to a narrow round of interlocking operations, the numerical work can be performed consistently. On testing the results by comparison with facts which have not been included in the theory, a few discrepancies and inconsistencies are found. An examination of these in detail would extend this notice far beyond the limits of a review and can not be attempted here.

It should be distinctly understood that Bigelow's fundamental equation starts with the thermodynamic equation which has been founded on the behavior of a definite volume of a gas or vapor enclosed in hypothetically non-conducting and impervious walls, when only the internal circumstances of pressure, density and temperature are changed; but in the adaptation of this equation to the free atmosphere, other terms are added which represent infringement of this condition by external interference, and still the process is called "thermodynamic." While an enlargement of the boundaries of this science is desirable, and while perhaps no better way can be found for extending these boundaries than by attending to the experiments which nature performs for us on a gigantic scale, still it must not be forgotten that by this new departure we have slipped our moorings and are at sea on an unknown ocean of many vague possibilities, where the precision of the old

thermodynamics may not be easy to retain. If this is recognized, then, knowing that meteorology has never been an exact science, there is no reason why this new departure should not be welcomed and thoroughly tested with whatever modifications and improvements may be found necessary; but warning should be given that the terms are now used in a new sense. The present examples go a long way toward supplying such tests. Bigelow's thermodynamics supplements the equally elaborate hydrodynamics of Vilhelm Bjerknes in a much-needed way. As in all other departments of meteorology, however, the application of the principle is more or less handicapped by the imperfection of the record. A single atmospheric sounding is never completely synchronous, and even if it were, the need remains for connecting the single air column with its surroundings. The importance of a study of cross currents at every level, in both velocity and azimuth, has been shown by Sir W. N. Shaw.

The method introduced in the first part of this work (pp. 262–292) for finding the solar constant, assumes that the reduction of high-sun and low-sun observations by the common, but incorrect, hypothesis (that the transmissive power of the atmosphere has no diurnal variation) will give the solar radiation at the 10,000-m. level, but not the radiant value outside the atmosphere; and that an approximately doubled interchange of thermal quantities above the 10,000 m. level (to which the author is led by his thermodynamics) is due to *reflection* of solar rays from the atmosphere at higher levels; whence the value of the solar radiation outside the atmosphere must be doubled. No one doubts that there is extensive reflection of solar rays by the air. The point to be carefully noted is that these rays can not be both reflected and absorbed. Hence, since any rays which are reflected can have no effect whatever on the thermal state of the substances in question from which the reflection proceeds, this hypothesis also is erroneous. The errors made in these two hypotheses have opposite signs, and it is conceivable that the opposite errors very nearly counterbalance each other, but the procedure is wholly empirical, and any approximation to the truth obtained in this way is accidental.

In spite of the foregoing assumption that all losses of solar radiation above the 10,000-meter level are by reflection, the seventh chapter introduces considerations of absorption and transmission of solar rays by the various 1,000-meter layers up to 90 km. Though inconsistent with the preceding hypothesis, this move is in the right direction. Unfortunately, however, both here and in earlier parts of this work, some serious errors occur. An extensive footnote (pp. 278 to 280) contains the following:

"*Transformation Factors.*

$$\frac{\text{kilogram}}{(\text{meter})^2} = \frac{\text{Gr.} \times 10^3}{\text{cm}^2 \times 10^4} = \frac{\text{Gr.}}{\text{cm}^2} \times 10^{-1}.$$

$$\left(\frac{\text{M.K.S.}}{\text{mech. units}}\right) \times \frac{10 \times 60}{41\,851\,000} = \frac{\text{Gr. cal.}}{\text{cm.}^2 \text{ min.}}.$$

Factor = 0.000014336."

The true derivation of the conversion factor for radiant energy from M.K.S. mechanical units into C. G. Min. thermal units is of course:

$$\frac{1 \text{ joule}}{\text{m}^2 \text{ sec.}} = \frac{1 \text{ kilogr. cal.}}{4185.1} \times \frac{1}{\text{m}^2 \text{ sec.}},$$

which is equivalent to

$$\frac{10^3 \text{ gram cal.}}{4185.1} \times \frac{60}{10\,000} = 0.0014337 \text{ (C. G. Min.)}.$$

The same error is repeated on page 377. On page 415 the author defends his mistake thus: "Since the Erg and the Joule are units of work they must refer to the unit volume and not to the unit area.

Hence Joule/*volume* $= ML^2T^{-2} \cdot L^{-3}$
$= ML^{-1}T^{-2} = 10^3 \times 10^{-2} = 10.$

While Joule/*area* $= ML^2T^{-2} \cdot L^{-2}$
$= MT^{-2} = 10^3 = 1{,}000.$"

It is quite true that, for example, the radiant energy received from the sun in 1 second on 1 cm.^{2} of surface represents the energy previously distributed through a volume of 3×10^{10} cm.^{3}; and that, when absorbed by a pyrheliometer, the temperature-effect is determined by the volume of water (equivalent to that instrument) which is heated. But in

measuring radiant energy by the pyrheliometer, we are dealing with the *transition at a surface* from radiant to thermal energy. What becomes of the thermal energy afterwards—whether it is distributed to a large or a small volume—does not concern us. The idea of volume is already implied in the definition of the calory which is the thermal effect of a heat unit (equivalent to an equal amount of radiant energy) upon unit volume of water.

The consequences of this mistake are momentous. Whole tables of figures covering several pages have been obtained with this erroneous transformation factor. Other insidious errors may be traced to this misconception. For instance, on page 126 we read: "The Kurlbaum coefficient of the Stefan formula for a perfect radiator is taken at 7.68×10^{-11} (C. G. Min. C°) $= 5.32 \times 10^{-6}$ joules per square meter per sec., so that the air radiates at six times the rate of a perfect radiator in the ether." Six times more than perfection is a rather large order.

In transforming the coefficient in the Stefan Law (on p. 279) by a formula with Planck's constants in C.G.S. units, Professor Bigelow has incorrectly divided by the number of ergs in a joule (10^7), obtaining (p. 280): Sigma $=$ 5.1210×10^{-12} in C.G.S. mechanical units, where the exponent should be -5. His value in M.K.S. mechanical units on this page is sigma $= 5.1210 \times 10^{-13}$, which involves an additional error in the logarithmic work. The correct value (after a small change in the adopted basal number), namely, sigma $=$ 5.510×10^{-8} (M.K.S.) is indeed given at the bottom of the page, but it has not been used on page 126, where still a new error appears. Evidently, the author's ideas on this subject are considerably mixed.

On page 370, the energy of solar radiation "used in heating" the atmospheric stratum "above 38,000 meters," is said to be "the true albedo of the earth's atmosphere," which implies that the author has a very obscure idea of what astronomers mean by "albedo." This is surprising, since he has given a correct definition of the word on page 277.

From what precedes, it results that the numerical values of thermal quantities in Tables 84 and 86, which are said to be in "gr. cal./cm.^{2} min." should all be multiplied by 100, in which case the sum of the "atmospheric thermal quantities attributable to solar radiation" would not bear the remotest resemblance to the "solar constant," an assumption which is fundamental to Bigelow's entire argument; nor would the so-called "total atmospheric radiant energy" be "equivalent to the 'solar constant' at the distance of the earth" (p. 385), since, even were the equivalence merely one of ultimate derivation, other factors must be considered, for example, the loss of solar radiation by reflection in passing through the air, which does not enter into the thermodynamic equation, notwithstanding the author's assertion (on p. 262) that the albedo of the earth "can be found indirectly by thermodynamic computations."

The summation of atmospheric thermal quantities from the surface to 35,500 meters is said to "give the amounts measured by the pyrheliometer" (p. 379); but the "free heat" $(Q_1 - Q_0)$ in each 1,000-meter layer, has been stated for unit mass (since the dimensions of heat, work and inner energy are given as $[L^2]$ on page 376, instead of $[ML^2T^{-2}]$, which requires that both M and T shall be unity), and therefore the numerical values should be multiplied by the density of the layer in transforming them to mass measurement. Moreover, $(Q_1 - Q_0)$ is due to a flux of long duration and therefore should be *averaged,* rather than summed, in order to compare it directly with the solar radiation at a given moment. Finally, although neither an average nor a sum of these quantities is exactly equal to the simultaneously exhibited solar radiation, the free heat, though derived from solar radiation, has no immediate connection with the solar radiation at the midday observation to which $\Sigma(Q_1 - Q_0)$ is incorrectly equated, for it continues by night as well as by day and represents the accumulated result of many days of sunshine, as the writer of this notice has shown in a paper "On the Solar Constant."[1]

[1] *American Journal of Science,* Vol. XXXIX., p. 201, February, 1915.

What the thermodynamic computations do show, however, is that the solar effect on the atmosphere above 35,500 m. is nearly as great as that on the lower layers; and this is a fact of very great importance.

The terms "absorption" and "transmission," as applied to the derivation of atmospheric free heat from the sun's rays on page 377, have apparently been transposed, but are correctly applied as regards terrestrial radiation. This follows because Q is the free heat "transmitted" in a way within the layer, but it is obtained by *absorption* of radiation and is proportional to such absorption, other things being equal.

The atmospheric heat on which depends the internal radiation of the atmosphere J_a, given for each 1,000 meters, is equal to the change in its radiant potential ΔK, and is due to absorbed terrestrial radiation. The curve of the change of density with the altitude agrees with that of the J_a function, showing that the expansile force of the air, or that force which gives its adiabatic expansion, is wholly governed by the mechanism of internal radiation between the air molecules. J_a is largest at the ground surface and progressively diminishes to the outer limit of the atmosphere. The free heat (Q), on the other hand, is distinct from J_a and is wholly devoted to expanding the air above the adiabatic rate. While the summations of the two from the surface to about 31 km. are very nearly equal, their distribution is quite different. Q is very small in the lowest layers and increases upward, but with wide fluctuations. In a general way Q follows the fortunes of the incoming solar rays, and while it may not be wholly dependent on their absorption, it appears to be very nearly so. There is no evidence that the curve of density agrees with the solar radiation at every level, as is asserted in Chapter VII.

If the air radiated like a black body, the radiation J_0 of any layer could be computed from the temperature by Stefan's Law. Summed, layer by layer, for the air column up to 50 km., $\Sigma(J_{0\text{-}1} - J_{0\text{-}0}) = -381.81$, while $\Sigma(J_{a\text{-}1} - J_{a\text{-}0}) = -136.75$, or the air radiation is about one third that of a black body. The figures, as thus stated, denote a thermal transference of so many gram calories per minute within a volume of five million cubic centimeters, and are derived from Bigelow's computation after correcting for the erroneous transformation factor; but they lend no support to his curious conclusion that air radiates six times better than the black body.

The pyrheliometric method for finding the solar constant which is described in Chapter V., is further extended and modified in Chapter VII., pages 388 to 394. This method is finally admitted to be discredited by experience, though its author does not recognize why this is so. A new and entirely different method of discussing pyrheliometric data is then developed (pp. 394 to 401) which, though empirical, appears to eliminate the influence of water vapor and altitude in the general means very well, making it possible to compare winter observations with summer, and also to determine a station correction to sea level. In this method the erroneous data derived from incorrectly interpreted thermodynamics are abandoned. The extrapolation of the resulting curve (A, Fig. 76) gives solar radiation = 3.22 at the height for which the writer's reduction of Violle's high-level observation gave 2.86 gram cal./cm.^{2} min.; and thus it appears probable that the value which Bigelow adopts for the solar constant (3.95) should be reduced to 3.6 gram cal./cm.^{2} min. A similar result follows from the writer's interpretation of the thermodynamic argument.

The formula for the mass of aqueous vapor in the upper air (pp. 342 and 373) is both complicated and erroneous. It gives a value about three times too large at 10,000 meters, ten times too large at about 24,000 meters, and increasingly greater at still higher altitudes.

The author's faith in the virtues of a formula is seen in his publication of some columns of figures which make such well-recognized constants as the mass of a hydrogen atom and the charge on an electron, variable. There are a few minor mistakes, such as the conversion of the charge on an electron through the use of an erroneous dimensional formula, but these will be readily recognized.

The work under review is one of extremes. It ascends to heights of excellence and brilliant achievement, and then breaks down, where one would least expect it, in errors and fallacies which seem inexcusable in one so gifted. It is greatly to be regretted that a book involving so much original work, and containing so much that is really valuable, should be marred by blemishes which prevent it from being regarded as an authority. Nevertheless, if the judicious reader will pardon these blemishes, the larger part of this treatise constitutes a monumental work of great erudition, and of elaborate and industrious research.

FRANK W. VERY

The International Rules of Zoological Nomenclature, with Appendix and Summaries of Opinions No. 1 to No. 56. T. O. SMALLWOOD, 3216 N St., Washington, D. C., September, 1915. 4to. 28 pp.

The ninth International Congress of Zoology met at Monaco, March, 1913. The full report was issued by the Imprimerie Oberthur, Rennes, France, 1914. Owing to the disturbances in Europe this report is practically inaccessible to students, and no separate copies of the rules in English appear to be available, nor does any provision seem to have been made for the republication of the rules separately. Hence the publisher of this pamphlet, with the approval of the secretary of the International Commission and the careful supervision of some of the American members, has provided what may be regarded as a trustworthy edition which may be obtained as above indicated.

A partial reprint of the rules without the opinions has been issued in French by M. Maurice Cossmann in the *Revue critique de Paléozoologie* for July and August, 1914, and in this connection a word of caution seems to be required. On page 14 of the separate copy of this rendition of the rules we read as follows under the following caption:

Autres Décisions du Congrès se Rapportant à la Nomenclature

A. Des exceptions à la loi de priorité pourront être admises:

1°. Quand un nom de Genre ou d'espèce devrait être transporté à un autre Genre ou à une autre espèce existants;

2°. Quand un nom a été employé pour un Genre pendant 50 ans, jusqu'à 1890, dans les travaux scientifiques, tels que monographies, catalogues scientifiques, etc.;

3°. Quand le nom le plus ancien, conformément à la loi de priorité, n'a pas été admis pendant 20 ans dans la systématique scientifique.

Note.—Chaque exception doit être soumise à la Commission internationale de Nomenclature qui examine chaque cas et le soumettra au prochain congrès international.

As the title of the pamphlet reads "Règles internationales . . . Adoptées par les Congrès," etc., it would seem that the reader might readily suppose that the paragraphs quoted from M. Cossmann's reprint (but not appearing in the English version) had been affirmatively decided by the congress. This, however, is not the case. Paragraphs *A*2 and *A*3 were submitted, it is true, but were definitely rejected, though no indication of this appears in the French reprint. Paragraphs *A* and *A*1 are qualified by the note under paragraph *A*3, each case to be submitted to the commission and decided on its merits.

WM. H. DALL

SPECIAL ARTICLES

THE LIGHT-SENSIBILITY OF COPPER-OXIDE

THE fact that selenium changes its electrical conductivity under the influence of light was discovered by May in 1873. Since that time the property of light-sensibility has been looked for in many substances and it has been found that sulphur, shellac, paraffin, anthracene and several other substances possess this property to a slight extent. The most noteworthy addition to the list was made by Jaeger who discovered the light-sensibility of stibnite (native Sb_2S_3) in 1907. Since a careful study of the behavior of these substances is bound, ultimately, to shed light on the mechanism of metallic conduction, it seemed worth while to continue the search for other substances which show marked light-sensitiveness. Recently the

writer found that copper-oxide (Cu_2O, presumably) shows the effect quite unmistakably.

Without going into details here as to the mode of production of copper-oxide cells or bridges, it may be stated that copper-oxide has a much lower specific resistance than either selenium or stibnite and is much the more transparent toward red light (layers having a thickness of more than 1 mm. are still slightly translucent). The fundamental facts which have been established for this new light-sensitive substance are:

1. The conduction is electronic and not electrolytic.

2. The increase in conductivity, occasioned by light, is distinctly different from that produced by a heating effect.

3. The conductivity increases with the applied voltage, *i. e.*, Ohm's law is not obeyed (voltage effect).

4. The region of increased conductivity spreads slightly to portions of the material not illuminated (transmitted effect).

5. The region of highest sensibility lies in the ultra-violet near $\lambda = 2800$ A. U.

6. Cooling in liquid air increases the percentage change in conductivity and displaces the sensibility maximum in the red toward shorter wave-lengths.

7. The relation between the radiant energy absorbed (E) and the resultant change in conductivity (C) is very approximately of the form $C = KE^{\beta}$ where K is a constant and β lies near 0.5.

While the percentage change in conductivity upon illumination is much less than that of selenium and stibnite, the comparatively high conductivity of copper-oxide makes the absolute increase quite large. The best cell which the writer has thus far constructed has a resistance of 15,200 ohms at 17° C. for 1 volt. The change in conductivity occasioned by the light from a 40-watt tungsten lamp at 20 cm. is about 15 per cent. The area exposed to radiation is about 12 mm.^{2} If this cell be connected to a 2-volt cell and a galvanometer (forming part of a simple potentiometer) a sensitive device for detecting radiant energy is produced. Exposing the cell to daylight in a moderately lighted room throws the galvanometer spot of light violently off the scale. Monochromatic radiations which are quite too feeble to affect a sensitive radio-micrometer, bring about large deflections when allowed to fall on the copper-oxide cell. If the cell be connected to a telephone receiver and battery and if an intermittent light beam of definite frequency be allowed to fall on the cell, a clear, musical note is heard.

The preceding discussion is to be looked upon as being of a preliminary nature. A systematic search for light-sensibility is being undertaken and a complete account of the work will appear later.

A. H. Pfund

Johns Hopkins University,
November, 1915

Radioactivity of Underground Waters in Providence and the Vicinity

Some idea of the distribution of radium salts near the surface of the earth may be obtained from a study of the relative amounts of radium emanation dissolved in underground waters. Within the last ten years a number of the better known springs and wells in America, Europe and Japan have been examined for emanation content. Some of the activities obtained have been tabulated by Schlunt and Moore[1] also by Dole.[2]

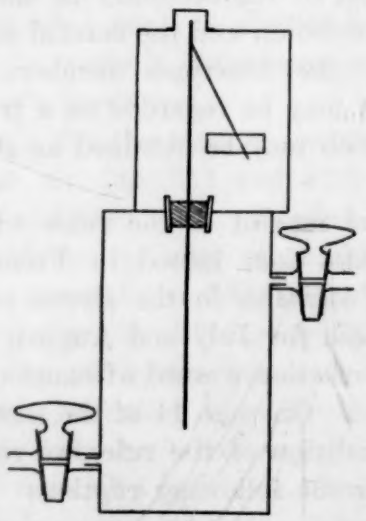

Fig. 1.

[1] U. S. Geological Survey Bulletin 395, 1909.

[2] U. S. Geological Survey, "Mineral Resources of the U. S., 1913," Part II., pp. 393–440.

The most active source thus far recorded was found in mine water at Joachimsthal, Bohemia, and contained per liter an amount of emanation equivalent to that in equilibrium with 700×10^{-10} gram radium.

Fig. 1 shows the type of electroscope used in the present experiments. The gold leaf was mounted on a brass rod passing through a sulphur plug into the ionization chamber. The leaf was used at a fairly high sensibility. Since it was not convenient to use a guard ring, the natural leak of the instrument was large, varying from .25 to .40 divisions per minute on different days. No suitable constant source of potential was available and the leaf was charged with an ebonite rod by removing the metal cap at the top. The ionization chamber was gas tight and was of approximately 725 c.c. capacity.

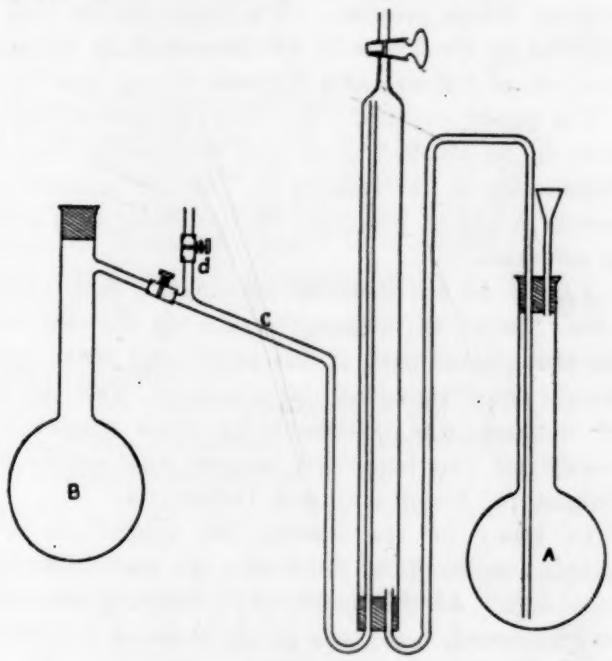

FIG. 2.

Samples of water to be tested were collected in suitable boiling flasks at different localities. The contained emanation was removed by boiling and allowed to cool. The ionization chamber was then partially exhausted, and the mixture of air and emanation allowed to flow slowly into it through a drying tube of calcium chloride with end plugs of cotton wool. The rate of motion of the gold leaf in divisions per minute after three hours was used in comparing the activities of different samples. All readings were corrected for the natural leak of the instrument and for the decay of the emanation itself during the time elapsing between collection and examination. After three hours the emanation was pumped off by means of a foot bellows through a long pipe into the open air far outside the laboratory. The ionization then decayed with the half value period of active deposit.

A diagram of the apparatus used for boiling out the emanation which is but a slight modification of that described by Makower and Geiger[3] is shown in Fig. 2. The burette was filled with water previously boiled in the flask *A*. The water under investigation in flask *B* was boiled from ten to fifteen minutes. Emanation still remaining in the connecting tube *c* after the burette had been emptied was carried over into it by air let in through the side tube *d*. 920 c.c. of water was collected for each test.

Standardization of the Readings of the Electroscope

In order to standardize the readings of the electroscope 10 c.c. of a standard solution containing 1.57×10^{-5} mg. radium was diluted to 250 c.c., and 60 c.c. of this solution boiled thoroughly and allowed to grow emanation for 31 days. This emanation, the equivalent to that in equilibrium with 3.77×10^{-9} gram radium, was introduced into the electroscope. The ionization after three hours was 57.11 divisions per minute.

The results obtained from a number of sources are given in the following table:

Column 5 in the above table records the amounts of radium emanation found in the different sources. These values are comparatively large. The maximum activity found in spring water by Shrader[4] near Williamstown, Mass., was 2.12×10^{-10} gram radium. The most active sources found during this investigation were in Arlington, R. I. Both the springs examined flowed out of Fenner's Ledge. This contains deposits of graphite mixed with a low-grade soft coal. The geological

[3] "Practical Measurements in Radioactivity," Longmans, 1912, p. 114.

[4] *Physical Review*, May, 1914, pp. 339–345.

Source	Locality	Approx. Cap. Liters per Min.	Corrected Div. per Min. per Liter.	Equivalent in Radium 10^{-12} Gram	Remarks.
Spring	Arlington, R.I.	10	87.78	57.93	Graphite mine.
Spring	Arlington, R. I.	6	70.80	46.71	Nr. car barn.
Spring	Nr. Wilbur Ave.	3	17.74	11.70	On Woonsocket car line.
Spring	Girard's Hatchery	30	16.98	11.19	Mineral Spring Ave.
Spring	Quinsnicket Park	5	15.65	10.33	Called Cool Spring.
Spring	Girard's office	.....	5.85	3.86	150-gal. tank.
Well	Nr. Bristol, R.I.	.. ..	4.21	2.78	60 ft. deep.
Spring	Smithfield Ave.	1	3.39	2.24	Nr. reservoir.
Spring	Johnson, R. I.	.....	3.34	2.20	Ochee.
Spring	E. Providence	2	1.78	1.18	On Lion Farm.
Well	College pump	.....	1.10	.73	On Campus.
Well	In heating plant	.....	1.03	.68	On Campus (unused).
Tap water	Eng. Lab.	.....	.05	.03	From city reservoir.

formation is such that the water comes in all probability from a considerable depth. Graphite is now being mined near the surface. Several samples of this graphite were powdered and tested qualitatively in an α ray electroscope. Only slight traces of radioactive content could be found.

I am indebted to Professor B. B. Boltwood, of Yale University, for the standard solution used in calibrating the electroscope.

Brown University, P. B. Perkins
June 14, 1915

SOCIETIES AND ACADEMIES

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

At the 489th meeting of the society, held October 19, 1915, Dr. D. S. Lamb read a paper on "The Medicine and Surgery of the Ancient Peruvians." They used Peruvian bark for fevers. It is doubted whether syphilis, leprosy and tuberculosis occurred among them, although some infer that skin tuberculosis caused the mutilations represented on their pottery. Three skin diseases peculiar to the ancient Peruvians were the *mirunta*, the *verrugas* and the *uta*. Smallpox, measles, scarlet fever and yellow fever were introduced by the whites. Goiter prevailed; also the *tabardillo*. The heads of their infants were deformed. They let blood and treated dislocations, wounds and fractures, and trephined the skull. Dr. E. L. Morgan and others who discussed the paper agreed with the speaker in thinking that trephining had probably begun with the idea of getting rid of the evil spirit but was continued for its observed curative value. The idea of ridding the patient of an evil spirit was common to all primitive peoples. In the Iroquois language, said Mr. J. N. B Hewitt, the expression used in case of sickness is, "It is biting me." Dr. C. L. G. Anderson held that the megalithic people who preceded the Incas also knew much about medicinal herbs. They made infusions, powders and ointments of them. Sulphur, salty earths and hot springs were used as cures of rheumatism and skin diseases. Sarsaparilla, coca and quina were local drugs used.

Daniel Folkmar,
Secretary

NEW ORLEANS ACADEMY OF SCIENCES

The regular meeting of the academy was held in Tulane University on Tuesday, October 18. Dr. Gustave Mann presided. The paper of the evening was by Professor O. M. Rosenwall on "Some Methods of Offense and Defense among Insects."

The paper outlined the orders of insects which were to be touched upon and the specific insects which were to be referred to. As far as possible, insects found in the state of Louisiana were used as examples.

Among all the methods mentioned, those which were "active in defense," made up the material for the greater part of the paper, and these were mainly the "repugnatorial glands." This means of defense was possessed by some species of nearly all the important orders, and mainly in Coleoptera, Hemiptera and Orthoptera.

In many of the insects the appendages are adapted as means of defense, *e. g.*, mandibles and front-legs. At this point, the "praying mantis" was discussed, being one of the common insects of this region.

Then followed the use of "stings" in connection with "poison-glands," and the following subjects were discussed briefly: "Poisonous Saliva," "The Repellant Fluid of Several Species of Coleoptera," "Phosphorescence" and "Protective Attitudes"; the paper closing with "The Means of Defense among Insect Larvæ."

An interesting discussion among members took place after the reading of the paper, and examples of the insects discussed were on exhibition. The academy then adjourned.

R. S. Cocks,
Secretary